A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1801.

ARTICLE I.

A Letter from THOMAS YOUNG, *M. D. F. R. S. Professor of Natural Philosophy in the Royal Institution, respecting Sound and Light, and in Reply to some Observations of Professor* ROBISON.

To MR. NICHOLSON.

SIR,

IN the supplement of the Encyclopædia Britannica, are inserted several excellent articles by Professor Robison of Edinburgh: one of them appears to require some public notice on my part, and I consider your valuable Journal as the most eligible channel for such a communication, especially as you have lately done me the honour of reprinting the paper which gave rise to the Professor's animadversions. But in the first place, I shall beg leave to recall the attention of your readers, by a summary enumeration, to the principal positions which I have in that paper endeavoured to establish.

1. Sound, as transmitted through the atmosphere, consists in an undulatory motion of the particles of the air, Sect. III. This is generally admitted; but as the contrary has even very lately been asserted, it is not superfluous to have decisive evidence of the fact. Professor Robison's experiment with the stop-cock furnishes an argument nearly similar.

2. A current of air, forced by a moderate preſſure through a cylindrical pipe, diverges the leſs as its velocity is leſs, Sect. II.

3. At a certain point the divergency of ſuch a current increaſes ſuddenly, and the current mixes with the ſurrounding air, Sect. II.

4. So far is ſuch a motion from ſpreading equally in all directions, that on every ſide of the current the air is urged more towards it than from it, Sect. II.

5. Sound, admitted through an aperture, does not by any means diverge equally in all directions, and is probably very weak except in directions nearly rectilinear. From poſition 2 and 4, and from experience, Sect. VI.

6. Sound probably decays in the duplicate ratio of the diſtance, Sect. VII.

7. A ſimilar blaſt of air produces nearly a ſimilar ſound, in organ pipes properly commenſurate, Sect. VIII.

8. Light is probably the undulation of an elaſtic medium, Sect. X.

A. Becauſe its velocity in the ſame medium is always equal.

B. Becauſe all refractions are attended with a partial reflection.

C. Becauſe there is no reaſon to expect that ſuch a vibration ſhould diverge equally in all directions, and becauſe it is probable that it does diverge in a ſmall degree in every direction.

D. Becauſe the diſperſion of differently coloured rays is no more incompatible with this ſyſtem than with the common opinion, which only aſſigns for it the nominal cauſe of different elective attractions.

E. Becauſe refraction and reflection in general are equally explicable on both ſuppoſitions.

F. Becauſe inflection is as well, and, it may be added, even much better explained by this theory.

G. Becauſe all the phænomena of the colours of thin plates, which are in reality totally unintelligible on the common hypotheſis, admit a very complete and ſimple explanation by this ſuppoſition. The analogy which is here ſuperficially indicated will probably ſoon be made public more in detail; and will alſo be extended to the colours of thick plates, and to the fringes produced by inflection, affording, from Newton's own elaborate experiments, a moſt convincing argument in favour of this ſyſtem.

9. The particles of air may be jointly actuated by two or more ſounds, and in this caſe, the ſeveral motions are to be added or ſubtracted, in order to find the actual joint motion, Sect. XI.

10. The grave harmonic produced by a major third is accompanied by a very audible twelfth. This circumſtance is explained, and the effect of ſubordinate notes and ſubaltern ſtops, on the quality of ſounds, is ſhown by figures, Sect. XI.

11. A noiſe returning every ſecond, if audible, would be a C. From Sauveur; with an experiment, Sect. XII.

12. A chord retains always the form of its initial vibration. From experiments, in favour of Euler's theorem, againſt the ſimple harmonic curve, Sect. XIII.

13. The

13. The vibration of a chord is scarcely ever performed in the same plane. Its revolutions, and its subordinate vibrations, may be rendered distinctly visible under the microscope, Sect. XIII.

14. If a chord be inflected at any point of aliquot division, the harmonic secondary note corresponding to that division will not be audible; an experiment contradictory to some theories of the origin and of the inseparable nature of harmonic sounds, Sect. XIII.

15. The human voice is analogous to the organ pipe denominated from it, which consists of a tongue piece without any commensurate tube: and the falsetto is probably formed by the upper orifice of the trachea assuming the functions of the glottis, Sect. XV.

16. A temperament of progressive imperfection is the most convenient for practical music, and is easily approximated by tuning six perfect, and six equally imperfect fifths, Sect. XVI.

From the detached nature of the subjects which I have here enumerated, and the imperfect state of these branches of the mathematics to which they refer, it would have been in vain to attempt a very perspicuous and detailed discussion of them. My researches on these subjects have been much interrupted, and probably will not be very shortly resumed; but if they are of no further use to any person, I shall not think my labour lost; for I flatter myself that the inferences, which they have led me to draw respecting the theory of colours, will throw new light on all the most interesting parts of optics, while by a comparison with the obvious inferences from Dr. Herschel's important discoveries, they will also lead to some material illustrations of the phænomena of heat.

I shall now trouble you with some remarks in reply to Professor Robison: the passage to which I allude is this:

"We are surprised to see this work of Dr. Smith greatly undervalued, by a most ingenious gentleman in the Philosophical Transactions for 1800, and called a large and obscure volume, which leaves the matter just as it was, and its results useless and impracticable. We are sorry to see this; because we have great expectations from the future labours of this gentleman in the field of harmonics, and his late work is rich in refined and valuable matter. We presume humbly to recommend to him attention to his own admonitions to a very young and ingenious gentleman, who, he thinks, proceeded too far in animadverting on the writings of Newton, Barrow, and other eminent mathematicians." Encyclop. Brit. Suppl. Art. Temperament, p. 652.

According therefore to the author of this article, I have in the first place taken the liberty of giving severe advice, to a young mathematician who had never asked it; secondly, this advice is equally applicable to my own presumption; and thirdly, Dr. Smith's treatise on harmonics is a work intitled to the highest praise.

I did in fact endeavour to shew, that the gentleman in question had overlooked the labours of some former authors relative to his subject, but I accompanied my remarks with nothing like admonition. After observing that at present a geometrician may very easily fancy he has made discoveries, when the same facts were known and forgotten long before he existed, I proceeded in these words:

" An instance of this has lately occurred to a young gentleman in Edinburgh, a man who certainly promises in the course of time to add considerably to our knowledge of the laws of nature. The tractory—was first described by Huygens: Bomie and Perks have shewn, that it is the involute of the catenaria.—The curve which the author calls a cycloid is the companion of a trochoid, and is only a distortion of the figure by which Newton had very simply and elegantly solved the same problem.—On the whole it appears, that this ingenious gentleman has been somewhat unfortunate in the choice of those problems, which he has selected as specimens of the elegance of the modern mode of demonstration." Essay on cycloidal curves, British magazine for April 1800. Is this an admonition? Or is there here any remark which can at all apply to my insensibility to Dr. Smith's merit in harmonics?

I have read Dr. Smith's work with attention, and I imagine, from the polite manner in which Professor Robison is pleased to speak of my essay, he will not hesitate to allow that I have understood it. I took it up with great expectations; these expectations having been completely disappointed, I thought it right to state my cool and unprejudiced opinion of its merits, in order to prevent a similar disappointment in others. It is impossible therefore that an " attention" to any " admonitions" of a general nature, wherever they may be found, can influence such an opinion; and so far only as I am supposed to be an incompetent judge on the subject of harmonics, can it be asserted that it was either blameable or superfluous for me to express that opinion. As a mathematician and an optician I value Dr. Smith highly, but I must still beg leave to affirm, that his whole book of harmonics contains far, far less information, than either of the articles Temperament and Trumpet in the supplement of the Encyclopædia.

I do not mean to be understood that this work is so contemptible, as not to contain the least particle of important matter; but it appears to me that its errors counterbalance its merits. The only improvement on which Professor Robison himself seems to set a high value, is the application of the phænomena of beats to tuning an instrument: on the other hand, I conceive that the misstatement relative to the non-interference of different sounds, is an inaccuracy which far outweighs the merit of Dr. Smith's share of that improvement. I have asserted, that " Dr. Smith has written a large and obscure volume, which for every " purpose but for the use of an impracticable instrument, leaves the whole subject of tem- " perament precisely where it found it," and that " the system proposed for his changeable " harpsichord is neither in that nor in any other form capable of practical application." Professor Robison on the contrary says, " we do not see how it can be disputed, that Dr. " Smith's theory of the beating of imperfect consonances, is one of the most important " discoveries both for the practice and the science of music, that have been offered to the " public. We are inclined to consider it as the most important that has been made since " the days of Galileo. We are obliged to call it his discovery. Mersennus indeed had " taken particular notice of this undulation of imperfect consonances, and had offered con- " jectures as to their causes; conjectures not unworthy of his great ingenuity. Mr. Sauveur " also takes a still more particular notice of this phænomenon, and makes a most ingenious

" use

"use of it for the solution of a very important musical problem." P. 652 and 651. Why then are we obliged to call it Dr. Smith's discovery, or indeed any discovery at all? Sauveur had already given directions for tuning an organ pipe by means of the rapidity of its beating with others. Mem. de l'Ac. 1701. p. 475. Ed. Amst. Dr. Smith ingeniously enough extended the method: but it appears to me that the extension was perfectly obvious, and wholly undeserving of the name either of a discovery or of a theory. If Professor Robison thinks otherwise, there is nothing further to be said; but in all probability Dr. Smith considered this improvement as a very inconsiderable part of the merit of his treatise. No doubt an organ may be more accurately tuned by counting the beats than by any other method, although it may be questioned, whether the advantage of counting the absolute frequency of the beats will ever practically compensate the tediousness of the process.

It remains to be considered, whether Dr. Smith's changeable harpsichord is or is not an impracticable instrument; for, whatever Signor Doria might exclaim, Dr. Smith himself does not recommend his scale for common use. It is the opinion of many unprejudiced practical persons, that all occasional introductions of different semi-tones is perfectly impracticable; and some who have heard the effect of Dr. Smith's instrument, have declared that to them it was by no means agreeable. And indeed if we pay sufficient attention to the passages and modulations of the greatest composers, we shall be convinced, that granting all possible dexterity in the performer, it would be absolutely impracticable to adapt them to an instrument, so different from that for which they were composed, as Dr. Smith's is from the common harpsichord. It may easily be conceived, that an organ very correctly tuned, as Mr. Watts's probably was, for a particular key, might appear "sopra modo bellissimo" in that key; but the sequel of the story shews literally what Dr. Smith has allowed, that his temperament is inapplicable to our instruments, since it was utterly impossible to sing with it in the key of E e s, a key of exceedingly frequent occurrence. I have been informed on the best authority, that Dr. Smith restricted the organist of Trinity College to such keys and modulations, as were best suited to the system by which the organ was tuned; and that organ, as well as the instruments which were made for Dr. Smith, has long been tuned according to the more common method.

I spoke of Dr. Smith's system with flattened major thirds as of no value, not with regard to its intrinsic merits, but because it was not intended for any instrument in common use; since in these instruments the difficulty is not so much how to divide the imperfection among the thirds and fifths of the same scale, as to proportion properly the imperfections of the thirds of different keys. Yet I do not mean it to be understood, that I can agree to the solidity of those foundations on which Dr. Smith has built his system for a single scale: although to Stanley and to Doria it might be pleasing, because its imperfections are far too small to offend the ear. Professor Robison justly observes, that different persons differ exceedingly in their estimation of the effect of the same temperament on different concords, and that much of this arises from their different dispositions: it appears therefore that Dr. Smith

Smith was too precipitate in laying down his principle for the comparison of the effects of temperament.

With respect to the system which I have proposed, Professor Robison thinks that the temperaments of several of the thirds which occur frequently are much too great. If we wish to form a judgment of any system of temperament, it must be by comparison with some other. It does not appear with what system Professor Robison would wish the comparison to be made, but he rather seems to incline to the equal temperament, although he gives directions for tuning by another. At any rate, no temperament of an interval can be said to be much too great, unless it be greater than that of the same interval in the system of equal temperament; for if any interval be made more perfect than this, some other similar interval must be as much less perfect. In my system, the only thirds perceptibly greater than those of the equal temperament, are the major thirds on E, A e s, B, C i s, and F i s, and the minor on C, C i s, F, G i s, B e s, and E e s. Of these none can be said to occur frequently, except the major third on E, and the minor on C. The sixths require no separate consideration. Now since the minor chord is intended to be less completely harmonious than the major, its character will be by no means materially impaired by this imperfection, which it would be somewhat difficult to remove. The third on E is not sharp enough to be very offensive, but in compliance with the usual practice of making this third somewhat more perfect than the interval of A e s and C, I have, in the method recommended for common use, made it a better third than that of the equal temperament. The directions given for tuning in § 68, and in § 80, of the article, are liable to far greater objections. For instance, the temperament of the IIIds on A e s and F i s, in the latter, is about .00880, or more than a comma and a half; which Professor Robison will readily allow to be "much too great" for any thirds; since he has asserted, with Dr. Smith and others, that the error of a comma would be intolerable. Mr. Maxwell has, however, very decidedly proved, in his Essay on Tune, that the greatest harmonists, Corelli, Tartini, and Giardini, have admitted very frequently the error of a comma in their most refined compositions. And I have the authority of several celebrated performers on stringed and wind instruments for asserting, that they take of choice the characteristic semi-tone, leading into the key note, considerably sharper than the same note is tuned on any keyed instruments, making an imperfection of nearly two commas in the relation as third of the dominant, which is the fundamental note of the chord: while in the mean time our theorists have been labouring, by the most complicated contrivances, to introduce notes into keyed instruments, which shall have exactly a contrary effect, by making the ascending semi-tone as wide a step as possible. On asking very lately the opinion of a practical musician of great eminence, and one who in every respect does honour to his profession, he decidedly agreed in the superiority of such a diminished semi-tone, and observed that the key of E derived a very elegant character from the usual method of tuning D i s as E e s, a minor third to C: hence, the IIIds E e s and G being very little tempered, the IIId on the dominant B must

be about a comma and a half too sharp. The fact is, that in this case the harmony is somewhat impaired in order to improve the melody. The interval of 15 to 16 is far too small to be distinctly conceived as commensurate, it possesses therefore no melody in virtue of the perfection of its ratio, and a certain elegance of expression is added by approaching to the natural and colloquial ascent of a voice by imperceptible degrees. It must, however, be confessed, that some excellent musicians prefer a purer harmony, and in this, as in all other matters of taste, considerable latitude must be allowed for the habits and predilections of individuals.

I am, SIR,

With great respect,

Your obedient humble servant,

THOMAS YOUNG.

No. 48, *Welbeck-Street,* *July* 13, 1801.

ERRATA IN THE PAPER ON SOUND AND LIGHT.

In Table XII, *for* E *b* 83810 *read* E *b* 84197; *for* .0011562 *read* .0010116. In Fig. 53 the E *b* (Q) is too near D, and the E *b* (Y) should be above instead of below it.

II.

On certain Points of Nomenclature. By a Correspondent.

To Mr. NICHOLSON.

SIR,

London, July 15, 1801.

THE gas at this time, whose effects on breathing it are an object of great curiosity among chemists and amateurs, denominated by its discoverer Dr. Priestley, *dephlogisticated nitrous gas*, and subsequently by the Dutch philosophers who investigated many of its properties, called *oxide of azote gas*, or *gaseous oxide of azote*, is now universally spoken of by the name *gaseous oxide*. The very ingenious and promising philosopher, and expert experimenter, whom the managers of the Royal Institution have engaged, has detected the mistake of Deiman, Troostwyk, &c. who found out that it was capable of supporting *inflammation*, but erroneously concluded, that it could not possess *respiration*. Mr. Davy thereupon very justly enjoys the honor of the discovery of this, and other new properties of this oxide; but my object now is only to observe, that the term *gaseous oxide* carries no useful and discriminating import, but what is worse than no import at all, it is very liable to produce mistake and confusion; as was the case with the terms *fixed air*, *aerial acid*, &c. formerly

formerly ufed. It appears from the analyfis of Deiman and Davy, that the oxide under obfervation confifts of above 0,37 of oxigen, and 0,63 of azote or nitrogen. Secondly, That *nitrous gas*, or nitrous oxide confifts of about 0,68 of oxigen, and 0,32 of azote. Accordingly Deiman diftinguifhes thefe two oxides by the names, *oxide of azote*, or of *nitrogen*, and *nitrous oxide*, which terms are little elfe but an inverfion mutually of their order, and befides have no fpecific import. Is it worth while to propofe the appellations, *fub-oxide of nitrogen*, or *azote*, and *oxide of nitrogen*, or *azote*, in place of the names in ufe? This method is analogous to the mode of the new nomenclature in its lateft improvements, as in the inftances fub-muriate, and muriate of mercury, fub-carbonate, and carbonate of alkali, fub-phofphate, and phofphate of lime, &c.

I am,

YOUR FRIEND.

III.

On the Use of Steam as a Vehicle for conveying Heat from one Place to another. By COUNT RUMFORD.

(*Continued from page* 160.)

IN order that clear and diftinct ideas may be formed of the various parts of this apparatus,—even without figures,—I fhall diftinguifh each part by a fpecific name: the veffel in which water is boiled in order to generate fteam—and which, in its conftruction, may be made to refemble the boiler of a fteam engine—I fhall call the *fteam boiler*:—The vertical tube, which, rifing up from the top of the boiler, conveys the fteam into the tubes (nearly horizontal), which are fufpended from the cieling of the room, I fhall call the *prime conductor*: to the horizontal tubes I fhall give the name of *horizontal conductors*, or fimply, *conductors of fteam*: and to the (fmaller) tubes, which, defcending perpendicularly from thefe *horizontal conductors*, convey the fteam to the liquids which are to be heated, I fhall, exclufively, appropriate the appellation of *fteam tubes*.

The veffels in which the liquids are put that are to be heated I fhall call the *containing veffels*.—Thefe veffels may be made of any form; and, in many cafes, they may, without any inconvenience, be conftructed of wood, or of other cheap materials, inftead of being made of coftly metals, by which means a very heavy expence may be avoided.

Each *fteam tube* muft defcend perpendicularly from the *horizontal conductor* with which it is connected, to the level of the bottom of the *containing veffel* to which it belongs; and, moreover, muft be furnifhed with a good brafs cock, perfectly fteam-tight; which may beft be placed at the height of about fix feet above the level of the floor of the room.

This

This *steam tube* may either descend *within the vessel* to which it belongs, or *on the outside of it*, as shall be found most convenient. If it comes down on the outside of the vessel, it must enter it at its bottom, by a short horizontal bend; and its junction with the bottom of the vessel must be well secured, to prevent leakage. If it comes down into the vessel, on the inside of it, it must descend to the bottom of it, or at least to within a very few inches of the bottom of it; otherwise the liquid in the vessel will not be uniformly and equally heated.

When the steam tube is brought down on the inside of the containing vessel, it may either come down perpendicularly, and without touching the sides of it, or it may come down on one side of the vessel, and in contact with it.

When several steam tubes, belonging to different containing vessels, are connected with one and the same horizontal steam conductor, the upper end of each of these tubes, instead of being simply attached by soldering to the under side of the conductor, must enter, at least one inch, within the cavity of it; otherwise the water resulting from a condensation of a part of the steam in the conductor, by the cold air which surrounds it, instead of finding its way back into the steam boiler, will descend through the steam tubes and mix with the liquids in the vessels below; but when the open ends of these tubes project upwards within the steam conductor, though it be but to a small height above the level of its under side, it is evident that this accident cannot happen.

It is not necessary to observe here, that, in order that the ends of the steam tubes may project within the *horizontal conductor*, the diameters of the former must be considerably less than the diameter of the latter.

To prevent the loss of heat arising from the cooling of the different tubes through which the steam must pass in coming from the boiler, all those tubes should be well defended from the cold air of the atmosphere, by means of warm covering; but this may easily be done, and at a very trifling expence. The horizontal conductors may be enclosed within square wooden tubes, and surrounded on every side by charcoal dust,—fine sawdust,—or even by wool; and the steam tubes, and prime conductor, may be surrounded first by three or four coatings of strong paper, firmly attached to them by paste or glue, and covered with a coating of varnish, and then by a covering of thick coarse cloth. It will likewise be advisable to cover the horizontal conductors with several coatings of paper, for if the paper be put on to them while it is wet with the paste or glue; and if care be taken to put it on in long slips or bands, wound regularly round the tube in a spiral line, from one end of it to the other, this covering will be useful, not only by confining more effectually the heat, but also by adding very much to the strength of the tube, and rendering it unnecessary to employ thick and strong sheets of metal in the construction of it.

However extraordinary and incredible it may appear, I can assert it as a fact, which I have proved by repeated experiments, that if a hollow tube, constructed of sheet copper $\frac{1}{20}$ of an inch in thickness, be covered by a coating of only twice as thick, or $\frac{1}{10}$ of an inch in thickness, formed of layers of strong paper, firmly attached to it by good glue, the strength

ftrength of the tube will be *more than doubled* by this covering. I found by experiments, the moft unexceptionable and decifive, of which I intend at fome future period to give to the public a full and detailed account, that the ftrength of paper is fuch, when feveral fheets of it are firmly attached together with glue, that a folid cylinder of this fubftance, the tranfverfe fection of which fhould amount to only one fuperficial inch, would fuftain a weight of 30,000 lbs. avoirdupois, or above 13 tons, fufpended to it, without being pulled afunder or broken. The ftrength of hemp is ftill much greater, when it is pulled equally, in the direction of the length of its fibres. I found, from the refults of my experiments with this fubftance, that a cylinder of the fize above mentioned, compofed of the ftraight fibres of hemp, glued together, would fuftain 92000 lbs. without being pulled afunder.

A cylinder, of equal dimenfions, compofed of the ftrongeft iron I could ever meet with, would not fuftain more than 66000 lbs. weight; and the iron muft be very good not to be pulled afunder with a weight equal to 55000 lbs. avoirdupois.

I fhall not, in this place, enlarge on the many advantages that may be derived from a knowledge of thefe curious facts. I have mentioned them now in order that they may be known to the public; and that ingenious men, who have leifure for thefe refearches, may be induced to turn their attention to a fubject, not only very interefting, on many accounts, but which promifes to lead to moft important improvements in mechanics.

I cannot return from this digreffion without juft mentioning one or two refults of my experimental inveftigations relative to the force of cohefion, or ftrength of bodies, which, certainly, are well calculated to excite the curiofity of men of fcience.

The ftrength of bodies of different fizes, *fimilar in form*, and compofed of the *fame fubftance*, or the forces by which they refift being pulled afunder by weights fufpended to them, and acting in the direction of their lengths,—*are not in the fimple ratio of the areas of their tranfverfe fections*, or of their *fractures*; but in a higher ratio; and this ratio is different in different fubftances.

The *form* of a body has a confiderable influence on its ftrength, *even when it is pulled in the direction of its length.*

All bodies, even the moft brittle, appear to be *torn afunder*, or their particles feparated, or fibres broken, *one after the other*; and hence it is evident, that that *form* muft be moft favourable to the ftrength of any given body, pulled in the direction of its length, which enables the greateft number of its particles, or longitudinal fibres, to be feparated to the greateft poffible diftance—fhort of that at which the force of cohefion is overcome,—before any of them have been forced beyond that limit.

It is more than probable that the apparent ftrength of different fubftances depends much more on the number of their particles that come into action before any of them are forced beyond the limit of the attraction of cohefion, than on any fpecific difference in the intenfity of that force in thofe fubftances.

But to return to the fubject more immediately under confideration.—As it is effential that the fteam employed in heating liquids, in the manner before defcribed, fhould enter the

the containing veſſel at, or very near, its bottom, it is evident that this ſteam muſt be ſufficiently ſtrong, or elaſtic, to overcome, not only the preſſure of the atmoſphere, but alſo the additional preſſure of the ſuperincumbent liquid in the veſſel; the ſteam boiler muſt, therefore, be made ſtrong enough to confine the ſteam, when its elaſticity is ſo much increaſed by means of additional heat, as to enable it to overcome that reſiſtance. This increaſe of the elaſtic force of the ſteam need not, however, in any caſe, exceed a preſſure of five or ſix pounds upon a ſquare inch of the boiler, or *one third part*, or *one half*, of an atmoſphere.

It is not neceſſary for me to obſerve here, that in this, and alſo in all other caſes, where ſteam is uſed as a vehicle for conveying heat from one place to another, it is indiſpenſably neceſſary to provide *ſafety valves* of two kinds;—the one for letting a part of the ſteam eſcape, when, on the fire being ſuddenly increaſed, the ſteam becomes ſo ſtrong as to expoſe the boiler to the danger of being burſt by it;—the other for admitting air into the boiler, when, in conſequence of the diminution of the heat, the ſteam in the boiler is condenſed, and a vacuum is formed in it; and when, without this valve, there would be danger, either of having the ſides of the boiler cruſhed, and forced inwards by the preſſure of the atmoſphere from without; or of having the liquid in the containing veſſels forced upwards into the horizontal ſteam conductors, and from thence into the ſteam boiler. This laſt mentioned accident, however, cannot happen, unleſs the cocks in ſome of the ſteam tubes happen to be open.—The two valves effectually prevent all accidents.

The reader will, no doubt, be more diſpoſed to pay attention to what has here been advanced, on this intereſting ſubject, when he is informed that the propoſed ſcheme has already been executed on a very large ſcale, and with complete ſucceſs; and that the above details are little more than exact deſcriptions of what actually exiſts.

A great mercantile and manufacturing houſe at Leeds, that of Meſſrs. Gott, and Co. had the courage, notwithſtanding the mortifying prediction of all their neighbours, and the ridicule with which the ſcheme was attempted to be treated, to erect a *dyeing houſe*, on a very large ſcale indeed, on the principles here deſcribed and recommended.

On my viſit to Leeds the laſt ſummer, I waited on Mr. Gott, who was then mayor of the town, and who received me with great politeneſs, and ſhewed me the cloth halls, and other curioſities of the place; but nothing he ſhewed me intereſted me half ſo much as his own truly noble manufactory of ſuperfine woollen cloths. I had ſeen few manufactories ſo extenſive, and none ſo complete in all its parts. It was burnt to the ground the year before I ſaw it, and had juſt been rebuilt, on a larger ſcale; and with great improvements in almoſt every one of its details. The reader may eaſily conceive that I felt no ſmall degree of ſatisfaction on going into the dyeing houſe, to find it fitted up on principles which I had had ſome ſhare in bringing into repute, and which Mr. Gott told me he had adopted in conſequence of the information he had acquired in the peruſal of my *ſeventh* eſſay. He aſſured me that the experiment had anſwered, even far beyond his moſt ſanguine expectations; and, as a ſtrong proof of the utility of the plan, he told me, that his next door neighbour,

neighbour, who is a dyer by profeſſion, and who, at firſt, was ſtrongly prejudiced againſt theſe innovations, has lately adopted them, and is now convinced that they are real improvements. Mr. Gott aſſured me that he had no doubt but that they would be adopted by every dyer in Great Britain in the courſe of a very few years.

The dyeing houſe of Meſſrs. Gott and Co. which is ſituated on the ground floor of the principal building of the manufactory, is very ſpacious, and contains a great number of coppers of different ſizes; and as theſe veſſels, ſome of which are very large, are diſtributed about promiſcuouſly, and apparently without any order in their arrangement, in two ſpacious rooms, each copper appearing to be inſulated, and to have no connection whatever with the others, all of them together form a very ſingular appearance. The rooms are paved with flat ſtones, and the brims of all the coppers, great and ſmall, are placed at the ſame height, about three feet, above the pavement: ſome of theſe coppers contain upwards of 1800 gallons; and they are all heated by ſteam from one ſteam boiler, which is ſituated in a corner of one of the rooms.

The horizontal tubes which ſerve to conduct the ſteam from the boiler to the coppers are ſuſpended juſt below the cielings of the rooms: they are made, ſome of lead, and ſome of caſt iron; and are from four to five inches in diameter; but when I ſaw them, they were naked, or without any covering to confine the heat. On my obſerving to Mr. Gott that coverings for them would be uſeful, he told me that it was intended that they ſhould be covered, and that coverings would be provided for them.

The vertical *ſteam tubes*, by which the ſteam paſſes down from the horizontal *ſteam conductors* into the coppers, are all conſtructed of lead, and are from $\frac{3}{4}$ of an inch to $2\frac{1}{2}$ inches in diameter; being made larger or ſmaller according to the ſizes of the coppers to which they belong. Theſe ſteam tubes all paſs down on the *outſides* of their coppers; and enter them horizontally at the level of their bottoms. Each copper is furniſhed with a braſs cock, for letting off its contents; and it is filled with water from a ciſtern at a diſtance, which is brought to it by a leaden pipe. The coppers are all ſurrounded by thin circular brick walls, which ſerve not only to ſupport the coppers, but alſo to confine the heat.

The rapidity with which theſe coppers may be heated, by means of ſteam, is truly aſtoniſhing. Mr. Gott aſſured me that one of the largeſt of them, containing upwards of 1800 gallons, when filled with cold water from the ciſtern, requires no more than *half an hour* to heat it till it actually boils!—By the greateſt fire that could be made under ſuch a copper, with coals, it would hardly be poſſible to make it boil in leſs than hour.

It is eaſy to perceive that the *ſaving of time* which will reſult from the adoption of this new mode of applying heat will be very great;—and it is likewiſe evident that it may be increaſed, almoſt without limitation, merely by augmenting the diameter of the ſteam tube: Care muſt, however, be taken that the boiler be ſufficiently large to furniſh the quantities of ſteam required. The *ſaving of fuel* will alſo be very conſiderable: Mr. Gott informed me that, from the beſt calculation he had been able to make, it would amount to near two thirds of the quantity formerly expended, when each copper was heated by a ſeparate fire.

But

But these savings are far from being the only advantages that will be derived from the introduction of these improvements in the management of heat: there is one, of great importance indeed, not yet mentioned, which alone would be sufficient to recommend the very general adoption of them. As the heat communicated by steam can never exceed the mean temperature of boiling water by more than a very few degrees, the substances exposed to it can never be injured by it. In many arts and manufactures this circumstance will be productive of great advantages, but in none will its utility be more apparent than in cookery; and especially in public kitchens, where great quantities of food are prepared in large boilers; for, when the heat is conveyed in this manner, all the labour now employed in stirring about the contents of those boilers, to prevent the victuals from being spoiled by burning to the bottoms of them, will be unnecessary; and the loss of heat occasioned by this stirring, prevented; and, instead of expensive coppers, or metallic boilers, which are difficult to be kept clean, and often stand in need of repairs, common wooden tubs may, with great advantage, be used as culinary vessels; and their contents may be heated by *portable fire-places,* by means of steam boilers attached to them.

As these portable fire-places and their steam boilers may, without the smallest inconvenience, be made of such weight, form, and dimensions, as to be easily transported from one place to another by two men, and be carried through a door-way of the common width, with this machinery, and the steam tubes belonging to it, and a few wooden tubs, a complete public kitchen, for supplying the poor and others, with soups; and also with puddings, vegetables, meat, and all other kinds of food prepared by *boiling*, might be established in half an hour, in any room, in which there is a chimney (by which the smoke from the portable fire-place can be carried off); and, when the room should be no longer wanted as a kitchen, it might, in a few minutes, be cleared of all this culinary apparatus, and made ready to be used for any other purpose.

This method of conveying heat is peculiarly well adapted for heating baths: it is likewise highly probable that it would be found useful in the bleaching business, and in washing linen. It would also be very useful in all cases where it is required to keep any liquid at about the boiling point for a long time without making it boil; for the quantity of heat admitted may be very nicely regulated by means of the brass cock belonging to the steam tube. Mr. Gott shewed me a boiler in which shreds of skins were digesting in order to make glue, which was heated in this manner; and in which the heat was so regulated, that, although the liquid never actually boiled, it always appeared to be upon the very point of beginning to boil.

This temperature had been found to be best calculated for making good glue. Had any other *lower* temperature been found to answer better, it might have been kept up with the same ease, and with equal precision, by regulating properly the quantity of steam admitted.

I need not say how much this country is obliged to Mr. Gott, and his worthy colleagues. To the spirited exertions of such men, who abound in no other country, we owe one of the proudest distinctions of our national character; that of being an enlightened and an enterprising people.

IV.

On the Identity of Galvanifm and Electricity. By a Correfpondent.

Edinburgh, June 20, 1801.

To Mr. NICHOLSON.

SIR,

I HOPE you will not confider me as one ftruggling againft the conviction of truth, when I inform you, that I have repeated Mr. Cruickfhank's experiment, of charging a vial with Galvanifm fuccefsfully; but am inclined to draw a very different conclufion from his, in confequence of the phenomenon that prefents itfelf, both during the performance of that experiment, and when it is enlarged and extended on the fame principle.

The mode that I firft ufed, was that of filling a common vial with falt water, which was plunged in another veffel alfo filled with falt water; though no great nicety was ufed, yet fome attention was paid to the furfaces of the two liquors being nearly upon a level, and by that means it very well reprefented an electrical vial, coated to the fame height within and without; then the infide was connected with one end of the pile, and the outfide with the other. After different intervals of time, fome longer, and fome fhorter, the longeft of which extended to feveral hours, the vial was lifted out in one hand intirely from the veffel in which it ftood, and the tongue applied to the wire connected with the infide; the conftant refult was, that whether it ftood for a longer, or fhorter period, a flight fhock, or rather tafte, was felt on the tongue. Expecting to increafe this trifling fenfation, by multiplying the number of the vials, I made a range of a dozen communicating all the infides, and all the outfides together by wire; after trying this mode by every means my imagination could fuggeft, I could perceive no increafe of power whatever, but only the flight fenfation, perceptible by means of one vial only.

The conclufion I am inclined to draw from the above facts is, that Galvanifm cannot be accumulated by the fame procefs with electricity, but that merely a fmall proportion of it can be communicated to fluids, and that confequently a marked diftinction fubfifts between them, the Galvanic charge probably not being a fiftieth part of the power refiding in the pile; whereas the electrical charge is oftener many hundred times beyond the power of the machine that imparts it; and that the former can be increafed only, by increafing the original power.

I have alfo repeated my former experiments of charging an electrical vial through the infulated pile; and to determine the matter with the greateft accuracy, I made ufe of Lane's electrometer; when its knobs were fet at a given diftance, by means of the graduated wire, the difcharge conftantly took place at the fame number of revolutions of the machine, which-ever wire was connected with the jar, provided the machine was in equally

equally good order, and the weather equally favourable; but whenever an increafe or diminution was obferved upon trying one wire, a fimilar change was perceived, upon changing it for the other.

I have alfo found, that plants cannot be altogether faid to be non-conductors of Galvanifm; by connecting one end of the pile with the ftem of a plant, and taking the other wire in the hand. When the tongue was applied to any of the leaves, even at a confiderable diftance from the ftem, a flight tafte was perceived; but though I allowed the fame plant to remain for above twelve hours with the two ends of the pile connected with different parts of it, it fuftained no injury whatever. It is to be obferved, that the pile was ftrong enough to give a very fevere fhock through feveral human bodies.

I think the refult of the above muft lead us to conclude, that though a fmall quantity of electricity may attend the phenomena of Galvanifm, (and there is fcarcely an operation in nature wherein a fmall quantity is not produced, and as you yourfelf have obferved, that the very motions of the human body, and the drapery that covers it, will produce fome:) yet that the principal phenomena of Galvanifm, viz. the fhock, the fpark, and the fnap, are not electrical, and we muft here ufe the fame mode of reafoning, that we would in chemiftry, that when one of the leading properties of any body was abfent, that it muft have loft it, either by combination with another, or by decompofition; but as you are inclined to attribute the want of the effect of attracting light bodies to the want of velocity in Galvanifm, I beg to be informed, whether Mr. Cruickfhank's very powerful machine affects the electrometer equally with an electrical vial of the fame ftrength, for I am at a lofs what to call velocity, if the ellifion of light from the air, producing a found to be heard in another room, and rufhing to a conductor at a diftance, be not inftances of velocity equal to any obferved in electricity.

With the greateft refpect, and thanks both to you and Mr. Cruickfhanks, for the readinefs with which you attended to my laft, and the full explanation you took the trouble to honour me with,

I am, SIR,

A FRIEND TO THE EXPERIMENTAL RESEARCH OF TRUTH.

V.

An Account of a new Eudiometer. By Mr. DAVY. *From the Journal of the Royal Inftitution.*

THE dependance of the health and exiftence of animals upon a peculiar ftate of the atmofphere, and the relations of this ftate to proceffes connected with the moft effential wants of life, have given intereft and importance to inquiries concerning the compofition and properties of atmofpheric air.

This

This elaſtic fluid has been long known to conſiſt chiefly of oxigen and nitrogen, mingled together, or in a ſtate of looſe combination, and holding in ſolution water.

A variety of proceſſes have been inſtituted with the view of determining the relative proportions of the two gaſes, but moſt of them have involved ſources of inaccuracy; and lately all, except two (the flow combuſtion of phoſphorus, and the action of liquid ſulphurets), have been generally abandoned.

Both phoſphorus and ſolution of ſulphuret of pot-aſh abſorb the whole of the oxigen of atmoſpheric air at common temperatures, and they do not materially alter the volume, or the properties of the reſidual nitrogen; but their operation is extremely flow; and in many caſes it is difficult to aſcertain the period at which the experiment is completed.

I have lately employed as an eudiometrical ſubſtance the ſolution of green muriate, or ſulphate, of iron, impregnated with nitrous gas; and I have found that it is in ſome reſpects ſuperior to many of the bodies heretofore uſed, as it rapidly condenſes oxigen without acting upon nitrogen; and requires for its application only a very ſimple and a very portable apparatus.

This fluid is made by tranſmitting nitrous gas through green muriate, or ſulphate, of iron, diſſolved to ſaturation in water *. As the gas is abſorbed, the ſolution becomes of a deep olive brown, and when the impregnation is completed it appears opaque and almoſt black. The proceſs is apparently owing to a ſimple elective attraction; in no caſe is the gas decompoſed; and under the exhauſted receiver it aſſumes its elaſtic form, leaving the fluid with which it was combined unaltered in its properties.

The inſtruments neceſſary for aſcertaining the compoſition of the atmoſphere, by means of impregnated ſolutions, conſiſt ſimply of a ſmall graduated tube, having its capacity divided into one hundred parts, and greateſt at the open end; and of a veſſel for containing the fluid.

The tube, after being filled with the air to be examined, is introduced into the ſolution; and, that the action may be more rapid, gently moved from a perpendicular towards a horizontal poſition. Under theſe circumſtances the air is rapidly diminiſhed; and, in conſequence of the dark colour of the fluid, it is eaſy to diſcover the quantity of abſorption. In a few minutes the experiment is completed, and the whole of the oxigen condenſed by the nitrous gas in the ſolution in the form of nitrous acid.

In all eudiometrical proceſſes with impregnated ſolutions, the period at which the diminution is at a ſtand muſt be accurately obſerved; for, ſhortly after this period, the volume of the reſidual gas begins to be a little increaſed, and, after ſome hours, it will often fill a ſpace greater by ſeveral of the hundred parts on the ſcale of the tube, than that which it occupied at the maximum of abſorption.

This circumſtance depends upon the flow decompoſition of the nitrous acid (formed during the experiment), by the green oxide of iron, and the conſequent production of a

* Dr. Prieſtley firſt obſerved this proceſs: for a particular account of it, ſee Reſearches, Chemical and Philoſophical, page 152. Johnſon.

 ſmall

ſmall quantity of aeriform fluid (chiefly nitrous gas) *; which, having no affinity for the red muriate, or ſulphate, of iron produced, is gradually evolved, and mingled with the reſidual nitrogen.

The impregnated ſolution with green muriate is more rapid in its operation than the ſolution with green ſulphate. In caſes when theſe ſalts cannot be obtained in a ſtate of abſolute purity, the common or mixed ſulphate of iron may be employed. One cubic inch of moderately ſtrong impregnated ſolution is capable of abſorbing five or ſix cubic inches of oxigen, in common proceſſes; but the ſame quantity muſt never be employed for more than one experiment.

A number of comparative experiments, made on the conſtitution of the atmoſphere at the Hotwells, Briſtol, in July, Auguſt, and September, 1800, with phoſphorus, ſulphurets of alkalies, and impregnated ſolution, demonſtrated the accuracy of the proceſſes in which the laſt ſubſtance was properly employed. The diminutions given by the ſulphurets were indeed always greater by a minute quantity than thoſe produced by phoſphorus and impregnated ſolutions: but the reaſon of this will be obvious to thoſe who have ſtudied the ſubject of Eudiometry. In no inſtance was it found that 100 parts in volume of air contained more than 21 of oxigen: and the variations connected with different winds, and different ſtates of temperature, moiſture, &c. were too ſmall, and too often related to accidental circumſtances, to be accurately noticed.

In analyſing the atmoſphere in different places, by means of impregnated ſolutions, I have never been able to aſcertain any notable difference in the proportions of its conſtituent parts. Air, collected on the ſea at the mouth of the Severn, on October the 3d, 1800, which muſt have paſſed over much of the Atlantic, as the wind was blowing ſtrong from the weſt, was found to contain 21 per cent. of oxigen in volume; and this was nearly the proportion in air ſent from the coaſt of Guinea, to Dr. Beddoes, by two ſurgeons of Liverpool.

If we compare theſe reſults, with the reſults gained more than twenty years ago, by Mr. Cavendiſh, from experiments on the compoſition of atmoſpherical air, made at London and Kenſington; conſidering, at the ſame time, the reſearches of Berthollet in Egypt and at Paris, and thoſe of Marti in Spain, we ſhall find ſtrong reaſons for concluding, that the atmoſphere, in all places expoſed to the influence of the winds, contains very nearly the ſame proportions of oxigen and nitrogen, a circumſtance of great importance; for, by teaching us that the different degrees of ſalubrity of air do not depend upon differences in the quantities of its principal conſtituent parts, it ought to induce us to inſtitute reſearches concerning the different ſubſtances capable of being diſſolved or ſuſpended in air, which are noxious to the human conſtitution: particularly as an accurate knowledge of their nature and properties would probably enable us, in a great meaſure, to guard againſt, or deſtroy, their baneful effects.

* The decompoſition of nitrous acid, by ſolutions containing oxide of iron, at its minimum of oxidation, is a very complex proceſs. The green oxide, during its converſion into red oxide, not only decompoſes the acid, but likewiſe acts upon the water of the ſolution; and ammoniac is ſometimes formed, and ſmall portions of nitrous oxide and nitrogen evolved with the nitrous gas.

VI. *On*

V.

On the Discolouration of Silver by Bird's Eggs, &c. In a Letter from ANTHONY CARLISLE, *Esq.*

To MR. NICHOLSON.

SIR,

I HAVE been lately occupied in tracing the natural history of bird's eggs, for a paper which has been in part laid before the Linnæan Society. Among other needful inquiries, I had occasion to make some experiments on the properties of the Albumen, Vitellus, and Gas contained in the *folliculus aëris* of eggs, which belong more properly to chemical history. As some of the results seem new, I beg leave to offer them for insertion in your Journal.

The well known fact of silver being discoloured by eggs, induced me to try which of the substances composing a fowl's egg is the cause of this appearance. I put the albumen, or white part of a new laid hen's egg, into a clean polished silver spoon, and the intire vitellus, or yolk, freed from its membrane, and all adhering albumen into another similar spoon. They were each exposed to a heat above that of boiling water, coagulated and reduced nearly to dryness, then left to cool, and twelve hours afterwards the silver remained untarnished in both.

A silver speculum placed in water, was adapted to receive the air contained in the folliculus aeris, which remained exposed to this gas for two days, without any discolouration.

I mixed the albumen and vitellus together in a spoon, and exposed them to heat without discolouring the silver. The intire albumen of a new laid egg was put into ten ounces (by measure) of boiling distilled water in a glazed pipkin, and kept till it was coagulated firmly: a clean silver spoon had been immersed with the albumen, to which it adhered in several places; after setting the vessel in a cool place for an hour, the silver immersed had acquired a deep blue and brown colour, with various prismatic shades.

An intire vitellus, freed from all other substances, was put into boiling water, together with a silver spoon as in the last described experiment, but the silver did not receive any tarnish.

Ten ounces by measure of the water in which the albumen of one egg had been boiled was filtered, and put into a glass vessel with a silver spoon; after remaining there for two hours it had received a deep copper colour.

The same quantity of water in which the vitellus of one egg had been boiled, was put into a glass with a silver spoon, and remained six hours without producing any change of colour. The water in the two last experiments was at 60° of Fahrenheit.

The

The matter which produced the discolouration of the silver proved to be sulphurated hidrogen, and it is worthy of remark, that this compound is not formed by the albumen of eggs without the addition of water; the water contained in the albumen itself seeming to be insufficient to give the hidrogen required, or its combination with this substance not admitting of decomposition *.

The smell of sulphurated hidrogen appears much stronger in the water where albumen has been boiled, if it have had silver immersed in it: perhaps the silver gives the predisposing affinities. That so large a quantity of sulphur should be contained in the rudimental materials of a bird, is a curious fact: but as sulphur is a substance still imperfectly known, it is difficult to say whence it is derived, and for what purposes it is employed.

The serum of the human blood contains sulphur, which in some experiments made for me by Mr. Fred. Accum yielded sulphurated hidrogen abundantly.

The air contained in the folliculus aeris, appears from the few experiments which I have made to be analogous to atmospheric air.

I am, dear SIR,

Your much obliged, &c. &c.

Soho Square, July 4, 1801. A. CARLISLE.

VI.

Inquiries relative to the Laws of Affinity. By CIT. BERTHOLLET. †

(Continued from page 158.)

Of the Metallic Solutions and Precipitates.

1. IN the two preceding Memoirs, I have principally considered those substances which are simple, and of which the composition is not variable; but it is known that the metallic oxides have different properties, according to the state of the oxidation; which must necessarily affect their chemical action.

The object of this memoir is to examine, what may be the influence of the degree of oxidation of the metals in their chemical action, and in general to compare this action with that of other substances, independantly of the properties with which I have been occupied (*Inquiries*, Art. XIII.)

* Perhaps the discolouring of silver spoons in eating boiled eggs, may arise from the water of the saliva being repeatedly brought into contact with the heated albumen, or from the water in which the eggs had been boiled having permeated the shell.

† Annales de Chimie, tom. XXXVIII. p. 113.

The solutions and precipitates of mercury have appeared to me to be particularly worthy of attention, because chemists have observed them with more care, and it is more easy to determine the conditions.

2. Fourcroy has described in the Memoirs of the Academy of Sciences, 1790, a sulphate of mercury, which he proves to be analogous to calomel, that is to say, formed by mercury little oxigenated. He has shewn that the sulphate of mercury, particularly when such a degree of heat only was employed as not to produce complete desiccation of the sulphuric acid and mercury, was divided by the action of the water into two sulphates, one of which may be called oxigenated sulphate, and the other mild sulphate. A simple method of obtaining this last combination, is to dilute by a nearly equal volume of water, the sulphuric acid which is treated with mercury, and to cause the mixture to boil; little sulphureous acid is formed, and instead of oxigenated sulphate of mercury, that sulphate is obtained of which the properties have been so well investigated by Fourcroy.

In the determination which Fourcroy has given of the constituent parts of the mild sulphate of mercury, he fixes at 0,05 the proportion of oxigen which is combined with the mercury; but for this determination, he supposes that on decomposing this salt by pot-ash, it is the pure oxide of mercury which is precipitated. But the metallic precipitates retain a portion of acid which he has neglected; I therefore believe, that according to his experiment, the proportion of oxigen ought to be rather greater than that which he establishes. I observe, that in many determinations made by chemists in modern times, this consideration has been neglected, which renders them in some measure uncertain.

The mild sulphate of mercury forms a permanent combination, which is not decomposed by water, as the oxigenated sulphate of mercury is, upon which I shall proceed to make some observations.

If the process indicated by Fourcroy be employed, that is to say, if the concentrated sulphuric acid be made to boil with the mercury, without letting it arrive at a state of desiccation, a white mass is obtained, composed of sweet sulphate and oxigenated sulphate. By the careful method of washing which he points out, the excess of acid that maintains the solution may be separated, from the oxigenated sulphate, and a portion of the mild sulphate.

When the operation is carried farther, or when a sufficient degree of heat is applied to the mild sulphate, a greater quantity of sulphureous acid is disengaged; the mercury becomes too much oxided to form the mild sulphate, and the combination is found intirely in the state of oxigenated sulphate, which varies in the quantity of sulphuric acid it retains. Let us examine it in the state it possesses when the operation has been carried to dryness, in which consequently it may be considered as not possessing an excess of acid.

In this state water produces a separation; the mass which was before white, turns yellow; the liquid becomes very acid, and holds a part of the sulphate in solution; there is formed what has been called, since the time of Rouelle, a salt with excess of acid, and a salt with the least portion of acid; but the proportions of these two combinations vary, 1. according

according to the quantity of acid that the firſt combination had retained; 2. according to the quantity of water employed; 3. according to the temperature; for the heat concurs with the action of the water.

If inſtead of ſimple water, an alkaline ſolution be employed, which exerts a more powerful action upon the acid than pure water, two different combinations are formed, one of which is found to be almoſt intirely the oxide, and the other in which the acid is for the moſt part engaged: the precipitate then afforded differs principally from that which water alone would have produced, by the ſmaller proportion of acid which it retains, and which depends on the degree of concentration of the alkaline liquor which acts upon it.

When the oxigenated ſulphate of mercury has preſerved a more conſiderable exceſs of acid, the action of the acid may be ſo weakened by the water, that no ſeparation will be made.

3. I have hitherto ſuppoſed, that there were but two ſulphates of mercury, one with the ſmalleſt poſſible proportion of oxigen, and the other with the greateſt proportion; but it is manifeſt that there are but theſe two extremes that are fixed, ſo that they may contain within their limits all the other degrees of oxidation: the properties of theſe intermediate combinations differ ſo much as to leave no room for the determination of their particular properties, unleſs both the degree of oxidation, and the proportion of the acid be known.

What I here obſerve as to the intermediate degrees of oxidation, muſt alſo be applied to the other metallic ſalts, ſuch as the ſulphate of iron, in which there are likewiſe only two fixed terms, that of the weakeſt and that of the ſtrongeſt oxidation.

4. Bergman had before diſcovered, that the ſolution of mercury by the nitric acid made in the cold, has different properties from that which is prepared by means of heat; and in his very excellent treatiſe on the analyſis of waters, he remarks, that the former does not ſo readily afford a precipitate with the ſolutions which contain ſulphuric acid, and that the precipitate it forms is white, whereas that of the ſolution made with heat is yellow; that the firſt then forms mild ſulphate of mercury, which is white, and more ſoluble in water than the oxigenated ſulphate, while the latter forms the oxigenated ſulphate.

When the nitrate of mercury is prepared by means of heat, ſome nitrous gas is at firſt diſengaged, but at a certain period, when the diſengagement is complete, the mercury is perceived to be diſſolved, with the production of ſcarcely any nitrous gas. I am indebted for this obſervation to Citizen Gay, a young chemiſt of the Polytechnic ſchool, who adds much ſagacity to conſiderable zeal. We ſee therefore, that by means of heat an oxigenated nitrate is formed, which if the operation be not ſtopped, afterwards combines with mercury, as in another operation the oxigenated muriate of mercury combines with a freſh quantity of this metal *. But in the fluid nitrate of mercury, there are no determinate proportions

* By an obſervation of Fourcroy it is ſhewn, that the oxigenated ſulphate of mercury ſubmitted to ebullition with water and mercury, acts alſo upon the metal: it muſt therefore paſs to the ſtate of mild ſulphate. The corroſive mercurial muriate, when diſſolved in water, does not act upon mercury; but by trituration with mercury without water, it begins to combine with it, and to impart its oxigen: the combination becomes uniform by ſublimation.

between

between the mercury moſt oxigenated, and that leaſt ſo: it appears that all the intermediate proportions may exiſt, as I have obſerved with reſpect to the ſulphates.

When the nitrate of mercury is precipitated by the muriate of ſoda, mercurial muriates are obtained, which differ according to the oxidation of the mercury. With the nitrate little oxided a white precipitate is obtained, which retains a part of the nitric acid, and cannot be diſſolved in the muriatic acid, nor be taken up by the concentrated nitric acid, without giving out much nitrous gas. From the ſolution prepared by heat, a precipitate is obtained rather of a yellow colour, which is not ſoluble in the muriatic acid, but readily diſſolves in the nitric acid, giving out little nitrous gas. The ſupernatant fluid affords by evaporation, a little corroſive mercurial muriate.

If the experiment be made with a nitrate, prepared in ſuch a manner that the mercury ſhall be in the moſt oxided ſtate, and has not re-diſſolved any metallic mercury, and if it be diluted with a conſiderably large quantity of water, no precipitate is formed, but all the mercury is found in the ſtate of corroſive mercurial muriate: nevertheleſs, it is not obtained in this ſtate but in a limited quantity, and ſometimes even none is had, according to the proportion of muriate of ſoda employed, becauſe the corroſive mercurial muriate has the property of forming with the nitrate of ſoda a quadruple ſalt.

This ſalt is capable of forming rhomboidal cryſtals, grooved on their face, of a conſiderable ſize: it fuſes upon ardent charcoal; by expoſing it to a ſufficient heat in a retort, all the mercury is ſeparated in the form of corroſive mercurial muriate. The reſidue is a nitrate of ſoda which retains a little muriatic acid, ſo that the ſeparation which takes place is decided by the reſpective volatility of the ſubſtances, and by a difference of affinity between the nitric acid and the muriatic acids relative to the oxide of mercury.

After the cryſtalliſation of this ſalt, another is obtained in ſmall needles, which appear to be a complex ſalt, in which the oxide of mercury is found in a greater proportion.

Nothing certain can be eſtabliſhed as to the reſults of the mixture of the nitrate of mercury highly oxided, and the muriate of ſoda, becauſe they vary according to the proportions of the ſubſtances which act.

From the preceding obſervations we deduce, that the nitric ſolution of mercury may hold this metal in ſolution from the loweſt degree of oxidation to the higheſt, or to that which is required for the conſtitution of corroſive mercurial muriate; that it may poſſeſs it in all the intermediate degrees, but that its properties will be different according to the degree of oxidation.

5. Fourcroy lays it down as a principle, that any metallic oxide whatever gives to acids a colour ſimilar to that which it has itſelf; whence he concludes, that when a mercurial precipitate which proceeds from a white ſalt acquires another colour, a change muſt have been made in the oxidation. This opinion does not appear to me to be well founded.

I took ſome muriatic acid, and diſſolved in it ſome red oxide of mercury; the ſolution was eaſily effected, without any diſengagement either of oxigen gas, or oxigenated muriatic acid: it ſpontaneouſly afforded fine cryſtals of corroſive mercurial muriate. I may here remark,

remark, that this procefs appears to me to be the moft fimple, and the leaft expenfive for the preparation of the corrofive mercurial muriate.

The combination which I had formed contained the red oxide of mercury, with all its oxigen; yet it was colourlefs; with ammonia it would have given a white precipitate, and with lime and the alkalis a precipitate more or lefs orange coloured.

The red oxide of mercury readily diffolves in the nitric acid, without any difengagement of oxigen; this folution cryftallizes and forms a white falt; but if there be not a fufficient excefs of acid, it gives with water alone a white precipitate; with a greater quantity of water recently diftilled, a yellow precipitate; with lime and the fixed alkalis a precipitate of a much deeper yellow colour.

A coloured oxide may therefore form white falts, and afterwards affume other colours, without undergoing any change in its oxidation.

6. Several chemifts have obferved, that the muriatic acid has a greater difpofition to combine with very oxided metals, than the nitric and fulphuric acids. Fourcroy applied this confideration with advantage to explain feveral phænomena. On this fubject he thus expreffes himfelf: (*Mem. de l'Acad.* 1790, page 381.) "Every acid requires quantities of oxigen in the metals, in order to combine with them; the muriatic acid in general does not combine with thofe metals, unlefs they be much loaded with this principle, or highly oxided: the mercury appears to be more oxided in the corrofive muriate than in the nitrate." I fhall take the liberty to make fome obfervations on the principles laid down by my learned colleague.

It does not appear to be exact to fay, that each acid requires different quantities of oxigen in the metals to combine with them; the nitric, the fulphuric, and the muriatic acids form combinations with mercury, from the leaft to the moft oxided terms; and it is the feries which refults in the muriates, which makes the principal difference in all pharmaceutical preparations, from the corrofive fublimate to the mercurial panacea: but there is this difference, that the combination of the fulphuric acid and the nitric acid with highly oxided mercury is much weaker, and more eafily decompofed, even by the action of water, than that of the muriatic acid, which on the contrary prefents a very permanent conftitution.

If our attention be directed to thofe metals which have the property of affuming large proportions of oxigen, fuch as iron, tin, antimony, &c. we fhall obferve the fame properties with regard to the fulphuric, nitric, and muriatic acids, fo that the action of the two firft, which diminifhes in proportion as the oxidation advances, is fometimes fo much weakened, that they abandon intirely, or are incapable of diffolving fome very oxided metals; on the contrary, the muriatic acid diffolves them and holds them in folution, fo that it cannot be perceived whether its action be weakened, or whether on the contrary, it be not increafed by a greater oxidation.

A plaufible explanation may be given of this comparative property of the fulphuric, nitric, and muriatic acids. Sulphur and azote, the bafes of the two former are faturated with oxigen,

oxigen, ſo that their reſulting affinity for the ſubſtances that are alſo greatly oxigenated is very weak; but the muriatic acid which appears to have in its conſtitution only a very ſmall proportion of oxigen, ought to have a much greater diſpoſition to combine with oxigenated ſubſtances.

7. In the preceding obſervations I have ſuppoſed, that the different alkalis ſhare the acid of a metallic ſolution with the oxide which is precipitated. I do not here ſpeak of precipitates by ammonia, which in ſome circumſtances is decompoſed, and by that means changes the conſtitution of the precipitate, as has been particularly ſhewn by Fourcroy; but this property is ſubject to modifications which demand particular attention.

The experiments of Bayen have aſcertained beyond a doubt, that the precipitates of mercurial nitrates and muriates retain a greater or leſs portion of acid, ſo that on expoſing the precipitates of the mercurial nitrate to a ſufficient heat, ſome nitrous acid is diſengaged; and on making the ſame experiment on the precipitates of mercurial muriate, a greater or leſs quantity of inſoluble mercurial muriate is ſublimed. I ſhall here add ſome obſervations to thoſe I have already made. (*Inquiries*, Art. XIII. No. 1.)

When the oxigenated muriate of mercury is decompoſed by the preciſe quantity of carbonate of ſoda, which is neceſſary to effect its precipitation, the precipitate contains muriatic acid, carbonic acid, and oxide of mercury in exceſs; ſo that on expoſing the precipitate to the action of heat, carbonic acid gas and oxigen gas are diſengaged; nearly half the mercury is reduced to metal, and the reſt of the precipitate is ſublimed, retaining all the muriatic acid which remained in combination with the oxide, and forming the mercurial muriate obſerved by Bayen. The liquid ſupernatant over the precipitate affords by evaporation, at firſt carbonate of ſoda, and afterwards a triple ſalt in which ſoda predominated.

The carbonate of pot-aſh preſents different phænomena. A very ſmall portion only is neceſſary to produce a total precipitation in the ſolution of corroſive mercurial muriate, and the precipitate obtained does not amount to half the weight of what would be given by the ſame quantity of oxigenated muriate of mercury, precipitated by the carbonate of ſoda. This precipitate, expoſed to the action of heat, gives out carbonic acid, and ſublimes almoſt totally in the ſtate of mercurial muriate, with the leaſt portion of acid; a very ſmall portion only reſumes the metallic ſtate. The liquid that covers the precipitate makes no efferveſcence with the acids; ſo that here the whole of the carbonic acid is combined in the precipitate with the oxide, and a portion of muriatic acid nearly double that contained in the precipitate by carbonate of ſoda. The liquid when evaporated affords a triple ſalt much more ſoluble in water than the oxigenated muriate of mercury: this ſalt cryſtallizes in ſilky needles.

The carbonate of ammonia cauſes an efferveſcence on decompoſing the corroſive mercurial muriate, and no acid is found either in the precipitate or in the ſupernatant liquid. The weight of the precipitate conſtitutes nearly five ſixths of the muriate decompoſed; a diſengagement of ammonia takes place on the addition of lime. When urged by heat, the ammonia is decompoſed, and azote gas only is received. All the mercury is ſublimed with-

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out reduction. But we perceive that in this sublimate the mercury cannot be as much oxided as in the sublimation of the precipitates by the fixed alkalis or lime, because it has lost a part of its oxigen by the decomposition of the ammonia. The liquid which covered the cipitate contained a combination of oxide of mercury, of muriatic acid, and ammonia.

The precipitation by ammonia presented the same phenomena, except the effervescence.

8. By directing our attention to the other metallic solutions and precipitations, the character of those of mercury are easily discovered, as well as the modifications which depend on the particular affections of each oxide; I therefore think we are justified in stating the following principles:

1. The acids act upon the metallic oxides the same as upon the other substances, in proportion to their mass, since when a metal has become but little soluble or insoluble, it may be dissolved by an excess of acid, or may form, by means of such excess, a more durable combination.

2. When a metallic combination is decomposed, the alkali or alkaline earth made use of, produces a division of the acid, in proportion to the energy of its action. If the metallic combination is weak, water is sufficient to decompose it; salts with either the maximum or minimum of acid are then formed. In this case the metallic oxides follow the same laws as other substances; but it sometimes happens that it is not the alkaline base which is added that assumes its share of the acid, but it is the metallic oxide on the contrary that divides the precipitate with the acid; as when we form fulminating gold, or the orate of ammonia. Sometimes also the precipitant, the acid, and the metallic oxide from two complex combinations; one of which is insoluble and the other remains liquid, as we have seen in the decomposition of the corrosive mercurial muriate by ammonia, and in the experiments that I have described (first series, Nos. X. and XI.)

In general, but particularly with respect to the metallic solutions, we must not separate in our reasonings, the substance employed by the name of precipitant from the liquid in which the precipitation is effected; but attention ought to be paid equally to all the substances presented, and which may form new combinations.

3. The coloured oxides may produce colourless combinations; but on giving out a part of the acid with which they were combined, their colour will again appear in proportion to the quantity of acid they have yielded; so that this colour is an indication of the constitution thus established, provided the state of oxidation has not been changed by any particular circumstance.

4. The metallic oxides cannot be compared together, unless they are taken in a determinate state of oxidation. All the combinations they are capable of forming vary, not only from this cause, but also from the proportion of acid they retain, when this proportion is not determined by a crystallization, With respect to oxidation there are only two extremes; that of the smallest, and that of the greatest oxigenation which can be considered as constant.

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Hence it follows, that the nomenclature can only indicate in a vague manner, and with great latitude, any metallic combinations in which the oxidation and the proportion of the acid are not determined.

5. The acids do not follow the ſame progreſſion in their affinity relative to the degrees of oxidation. There are ſome in which the affinity diminiſhes with the oxidation; ſuch are the nitric and ſulphuric acids; in others again it appears to increaſe, ſuch as the muriatic acid. Hence we may perceive, independant of all the conſiderations I have mentioned, how groundleſs the pretenſion was of claſſing the affinities of metals for the different acids, by conſidering them as conſtant forces.

6. The reſults of the complex affinities of metallic ſolutions, mixed with other ſalts, may alſo vary from the proportion of thoſe ſalts, conformably to what has been obſerved (*on the influence of proportion in the complex affinities*); ſo that then the proportion of oxigen in the metallic oxide, that of the oxide in the ſolution, and that of the ſaline combination brought into action with it, all contribute to the new combinations which are eſtabliſhed.

7. We might therefore obſerve in the chemical action of the metallic ſolutions, the laws which we have eſtabliſhed in that of the other combinations, if oxidation did not cauſe a change in the affinity of the metal, either for the acids, or for the other ſubſtances, and multiply, as it were, in the metals the property of forming combinations; whence the reſults are often ſo complicated, that though by accurate obſervation we may develope the facts and the circumſtances which determine them, yet they cannot be foreſeen by theory, from the mere conſideration of the known properties, as may be done with reſpect to other ſubſtances, the action of which depends on a ſmaller number of conditions.

8. The ſtate in which oxigen is found combined with the metals, has likewiſe an influence upon the properties of the metallic oxides, and of the precipitates compared with each other. Bayen obſerved, that ſeveral mercurial precipitates, as well as the red oxide, detonate ſtrongly, though in an unequal manner when expoſed to heat, after having been mixed with ſulphur, and that ſome do not poſſeſs this property; but he has not given the reaſon of the phænomenon and the exceptions. It appears to me indubitable, that this property of the oxide of mercury, and of the precipitates in which it predominates, is a conſequence of the oxigen poſſeſſing more caloric, than it preſerves in the combination that it forms with the ſulphur, or in the ſulphuric acid. The ſame thing therefore happens as with the nitrate and oxigenated muriate of pot-aſh; except that the effect is leſs conſiderable; but in the precipitates of the corroſive mercurial muriate, part only of the mercury can be conſidered as not combined with the muriatic acid, namely, that part which may be reduced into metal by the action of the heat which produces detonation. Such of the precipitates therefore as retain a ſufficient quantity of the muriatic acid, to admit only of a ſmall reduction of mercury by the action of heat, cannot produce detonation; and ſuch in fact are the precipitates by ammonia, by the carbonate of ammonia, and by the carbonate of pot-aſh.

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9. The property discovered by Bayen is therefore analogous to the fulminating property of the orate and the argentate of ammonia; but it is not perceptible in the other metallic oxides and precipitates. Hence the oxigen in the latter must necessarily contain a less portion of caloric.

10. The properties which depend upon oxidation vary therefore in each metal, on account of the proportions of oxigen; and those which depend on the state of concentration possessed by oxigen, constitute the leading cause of those phænomena which arise from the changes of combination in elastic substances. (*Inquiries*, Art. XIV. Nos. 12 and 15.)

(To be continued.)

VIII.

Experiments and Observations on various Kinds of Candles, made of Wax, Tallow, Spermaceti, and of such as are manufactured of a Mixture of these Substances; as well with Regard to Cheapness as to the Quantity of Light they afford. By Sig. FR. HERMSTAEDT, *Professor and Member of the Royal College of Physicians at Berlin*.*

THE illumination of apartments is an article of the first necessity for every class of people, and the means by which it is produced are so likely to advance in their price, that this illumination continues to be an object of considerable importance, for every family in housekeeping, whether numerous or moderate. But as it is impossible to obviate this necessity for lighting our apartments, it becomes the duty of every one to be attentive to the prospects of œconomy in this respect: since the saving, though small with regard to a single candle, may nevertheless in time amount to a sum by no means contemptible in housekeeping upon a larger scale; more especially, when the elegant enjoyments of life demand a brighter light, than is necessary for the mere purposes of domestic utility.

The experiments which I lately had an opportunity of making during the official examination of various sorts of tallow candles, with regard to œconomy in their consumption, as well as to the quantity of light afforded by their combustion, afforded results, which appeared deserving of attention, and were sufficient to induce me to make a new set of experiments, with candles made of mixed ingredients, in order to compare them with the former. These, likewise, furnished results by no means uninteresting, and afford conclusions of equal utility. These reasons have induced me to publish my observations for further examination; and even should my remarks and observations appear to some readers of very subordinate importance, yet I might request them to consider, that even facts of little moment have frequently afforded a foundation to more discoveries of greater magnitude.

* Translated from the German in *Scherer's Allgemeinen Journal der Chemie*, Vol. IV. page 40.

Firſt experiment with a wax candle of the ſize of ſix to a pound.—A white wax candle weighing exactly 2 ounces 11 drachms ($5\frac{3}{8}$ *Loth.*) *avoirdupois weight* *, was lighted and kept burning for one hour in a calm place; after which combuſtion it exhibited a loſs of weight of 115 grains. According to this proportion the combuſtion of the whole candle would have required 11.217 hours, or which is the ſame, 11 hours and 13 minutes. The burning of this candle proceeded quietly, with a tranquil, not undulating flame, and without notable ſmoke and ſoot.

Second experiment with a mould tallow candle of ſix to the pound.—The candle weighed 2 ounces, 10 drachms, ($5\frac{1}{4}$ *Loth.*) and loſt in weight 185 grains after an hour's burning; the whole of it would therefore have been intirely conſumed in 6 hours and 45 minutes. The flame was ſlightly agitated, and emitted much vapour.

Third experiment with a mould tallow candle of eight to the pound.—The weight of this candle was 1 ounce, 15 drachms, and 4 grains ($3\frac{7}{8}$ *Loth.*) When it had burned one hour, it had loſt 140 grains; whence it might have burned 6 hours and 40 minutes. The other phænomena during its combuſtion were like thoſe of the preceding.

Fourth experiment with a mould candle of nine to a pound.—Its weight was 1 ounce 12 drachms, ($3\frac{1}{2}$ *Loth.*) and the loſs of weight after one hour's burning was 140 grains; conſequently this candle would have laſted ſeven hours before its intire conſumption. The flame reſembled that of the laſt.

The reſults afforded by theſe experiments appear to me deſerving of attention in various points of view. It was on this account that I reſolved to make various kinds of candles, compoſed of pure ſpermaceti, of ſpermaceti and tallow, and of tallow and wax, in various proportions of the ingredients. And laſtly, I made trial of a mixture of wax, tallow, and ſpermaceti. The reſults which I obtained in theſe experiments were the following:

Fifth experiment with a mould candle, made of two parts of tallow and one of wax.—This candle weighed $2\frac{1}{2}$ ounces, 24 grains (5 *Loth.*) It was lighted, and after an hour's combuſtion loſt 166 grains; it might conſequently have burned for the ſpace of 7 hours and 22 minutes. The flame of this candle was a very good one; it emitted ſcarcely any vapour, and in this reſpect it very much reſembled the candle made of pure wax.

Sixth experiment with a candle manufactured of one part wax and three parts of tallow.—The weight of this candle was 2 ounces and a half, and 34 grains. From the loſs of 193 grains, which it ſuſtained by burning one hour, it appears that it would have required 6 hours and 23 minutes to be wholly conſumed. The flame of this candle was likewiſe ſteady, and afforded but a ſmall quantity of ſoot.

Seventh experiment with a mould candle made of pure ſpermaceti.—I made a candle of this deſcription, of the weight of 2 ounces and 3 drachms, ($4\frac{3}{8}$ *Loth.*) When lighted and kept

* The *Berlin pound* of 32 Loths, or 16 ounces, is equal to 3616.3 Engliſh grains; ſo that one ounce Berlin is equal to 226, and the drachm to $14.\frac{1}{8}$ Engliſh grains, diſregarding the ſmall decimal fraction of 3-10ths of a grain in the pound. The ſubdiviſion of the Engliſh avoirdupois weight into 16 ounces, each into 16 drachms has been employed here without farther reduction.---Tranſl.

burning

burning for one hour, it loſt 250 grains of weight, ſo that the whole time of its combuſtion would have amounted to 4 hours, 12 minutes. Whence it follows, that the candles made of ſpermaceti are far leſs œconomical with regard to expence, than wax or tallow. Beſides which, the flame produced by this candle was very unſteady, fleeting, and it alſo gave out a quantity of ſmoke and ſoot.

Eighth experiment with a candle compoſed of equal parts of ſpermaceti and tallow.—This candle weighed 2 ounces, 6 drachms, and 4 grains, ($4\frac{3}{4}$ *Loth.*) I ſuffered it to burn one hour, and found it to have loſt 213 grains; whence the whole time of its combuſtion would have been 5 hours 22 minutes. Its flame differed very little from that obſerved in the foregoing experiment.

Ninth experiment with a candle made of one part ſpermaceti and two parts of tallow.—The weight of a candle of this mixture was 2 ounces, 6 drachms, and 5 grains, ($4\frac{3}{4}$ *Loth.*) of which it loſt 200 grains by one hour's burning. Its total combuſtion, therefore, would have been accompliſhed in 5 hours, and $43\frac{1}{2}$ minutes. This candle guttered or overflowed very much, and afforded a bad, ſooty flame.

Tenth experiment with a candle prepared of one part of ſpermaceti mixed with three parts of tallow.—This candle weighed exactly 2 ounces, 6 drachms. and 45 grains, ($4\frac{3}{4}$ *Loth.*) By burning for one hour, it ſuſtained a loſs of weight of 235 grains; hence the whole time of its combuſtion would have amounted to 5 hours and 3 minutes. The flame of this candle alſo was not notably different from that of the laſt.

Eleventh experiment with a candle compoſed of equal parts of wax and ſpermaceti, together with three parts of tallow.—The weight of this candle amounted to 1 ounce, 6 drachms, and 56 grains, ($3\frac{1}{4}$ *Loth.*) During the combuſtion for one hour it loſt 246 grains, and would conſequently have laſted 3 hours, 54 minutes, before it was intirely conſumed. In other reſpects the flame of this candle was very bad; and it depoſited a quantity of ſmoke and ſoot.

Computation of the Expence attending the Conſumption of theſe different Kinds of Candles compared with each other.

In order to obtain a very accurate proportion of the ſaving of expence that might be made by burning theſe various ſpecies of candles, it would be neceſſary, not only that the candles themſelves ſhould conſiſt of equal maſſes, but likewiſe of equal diameters or circumferences: for ſince, as I ſhall prove in the ſequel, it reſults from my experiments, that the ſaving in the burning of a candle is proportional to the ſmallneſs of its diameter, and ſince the candles employed in experiment 3 and 4 had a ſmaller diameter than the others, the following calculation with regard to the œconomy in burning theſe two ſpecies, is only an approximation; whereas it is perfectly accurate with regard to all the others, becauſe they were uſed of exactly the ſame diameter. It will be eaſily perceived, that I have calculated the ſaving to be made in the burning of a candle from the quantity of fuel

or combuſtible matter, which is conſumed in a given time. Beſides which, I have aſſumed for the given time the ſpace of an hour; and in order to provide myſelf with a determined point of compariſon, I have taken that quantity of fuel which is conſumed in one hour by burning a wax candle, for the given unity as a baſe, and have thus compared the reſpective loſſes in the quantities of combuſtible matter, which the other ſorts of candles ſuſtain, while burning during an equal ſpace of time. The reſults of this calculation are, for the ſake of brevity, placed together in the following table:

If, in a given time, are conſumed of a pure wax candle - - 1000 Pounds:

There will be conſumed during an equal time of

a . . . tallow candle, 6 to a pound	- - - - -	1,608
b . . . ditto 8 ditto	- - - - -	1,217
c . . . ditto moulded, 9 ditto	- - - - - -	1,043
d . . . candle made according to the 5th experiment	- -	1,443
e . . . ditto - - - - 6th ditto	- -	1,678
f . . . ditto - - - - 7th ditto	- -	2,174
g . . . ditto - - - - 8th ditto	- -	1,852
h . . . ditto - - - - 9th ditto	- -	1,739
i . . . ditto - - - - 10th ditto	- -	2,008
k . . . ditto - - - - 11th ditto	- -	2,138

From theſe reſults it is very manifeſt, that of all candles thoſe made of pure and white wax burn the longeſt, and alſo that tallow candles are the more ſparingly conſumed, the thinner they are, or the ſmaller their diameter is, and that, on the contrary, ſpermaceti candles undergo the greateſt waſte of fuel; beſides, that they emit more vapours than candles of tallow, although thoſe vapours have not a very diſagreeable ſmell. He therefore, who deſires to illuminate his room with œconomy, ſhould procure very thin, mould tallow candles, ſo that about 12 or 16 of them will go to a pound, and he will find them ſtill more advantageous, than is ſhewn by the above reſult; ſince, in it, mould candles of only 9 to the pound have been reckoned. But even in that caſe we ſuppoſe, that in a certain rate of houſe-keeping 100 pounds of tallow candles of the ſize of 6 to a pound are required, the ſame illumination may be effected, according to the experiment here alluded to, with only 64¾ lb. if dipped candles of 9 to a pound be employed. Now the pound of mould candles coſts 5 *groſchen* *; conſequently 100 pound will coſt 20 dollars, 20 *groſchen*, whereas 64¾ lb. of dipped candles, reckoning the pound at 4½ *groſchen*, will coſt no more than to 12 dollars 3 *groſchen* 4½*d.* ſo that in ſuch a houſekeeping there is a yearly ſaving of 8 dollars, 16 *groſchen* 7½*d.* which in another upon a larger ſcale will come to a conſiderable amount. It need not be mentioned, being obvious of itſelf, that in thinner candles the thickneſs of the wick muſt always bear due proportion to the maſs of tallow.

* The Berlin *groſche* is equal to 1 4-15ths of a penny, or nearly one penny and a farthing Engliſh. The groſchen is ſubdivided into 12 pennies, and one Berlin dollar is worth 2 ſhillings 6 2-5ths. pence at par.

4 We

We may now from the preceding results, founded on experience, draw the following conclusions:

a) That in general tallow candles of a smaller diameter are consumed more sparingly than thicker ones.

b) That the reason of this phænomenon must be sought for in the mass of tallow, which being too great in thick candles, is heated at once, and thus volatilized without burning like the fat in roasting meat; for which reason,

c) Since a sufficient quantity of oxigen gas from the atmosphere cannot then act upon it, but only on the exterior part of the flame, a great part of the combustible matter must be lost in these vapours, at the increasing intensity of heat, without decomposing the air in order to extricate light by burning.—But this on the other hand,

d) Cannot be the case with thinner candles, because much less of fuel is lost, and also because in that case less of tallow is necessary to be consumed, in order to produce the same quantity of light as is afforded by a thicker candle: for in this case more of oxigen gas is decomposed, and more free light separated, which also

e) Affords this great advantage, that such a candle produces less vapour and soot, because the fuel being then more effectually consumed, will be volatilized, and go off as soot in an undecomposed state in a much less proportion.

But candles would afford the brightest and purest light, which have a broad, ribbon-like wick instead of a round one, or to which the form of hollow cylinders were given, so that the air might act upon the flame inwardly as well as outwardly. For by an arrangement of this kind all the combustible matter will be consumed, and no smoke nor soot, but merely water and carbonic acid gas can be formed, as must be evident to every one acquainted with the principles of chemistry. But to such as are not chemists, Argand's lamp may serve as a proof. At the same time it is equally evident from the preceding theory or explanation, that every flame, which is not obscured by soot, must afford a stronger light than another in opposite circumstances; and it is equally obvious, that such a flame, from the total absence of smoke and soot, cannot emit any unpleasant odour. I intend to institute further experiments on this subject, and to communicate them at a future time.

Lastly, I have also endeavoured to ascertain the intensity of light emitted by the various sorts of candles here mentioned. For this purpose the lighted candle was placed upon a table in a room totally darkened. I then, holding a book in the hand, withdrew backwards, until the characters became invisible to my eyes. The several distances from the luminous cone were accurately measured, and afforded the following results:

The wax candle (Experiment I.) emitted light, of which light the illuminating distance was 10 feet, 1 inch, 6 lines Rhinland measure *.

* A Rhinland foot is equal to about 1 foot and 3-10ths of an inch English measure.---A line is one-twelfth of an inch.---Transl.

The

	Feet	Inch.	Lines.
The tallow candle of Experiment II. gave - -	11	2	6
The ditto ditto III. - - -	11	5	0
The drawn candle of Experiment IV. - - -	12	2	3
The candle of ditto V. - - -	12	9	0
The ditto ditto VI. - -	11	3	0
The ditto ditto VII. - - -	9	9	0
The ditto ditto VIII. - - - -	12	6	0
The ditto ditto IX. - - -	12	4	6
The ditto ditto X. - - -	12	6	0
The ditto ditto XI. - - -	12	9	0

If these distances be compared with each other, and if the intensity of illumination shewn by the wax candle be taken as the standard unity, the following intensities of light, as produced by the several candles, will be the result by calculation.

If the illumination, effected by the burning of the wax candle according to Experiment I, be - - - - - - - - - 1,000

then that of Experiment II. will be	- - - - - -	1,107
ditto	III. - - - - - -	1,127
ditto	IV. - - - - - - -	1,203
ditto	V. - - - - - -	1,260
ditto	VI. - - - - - - -	1,104
ditto	VII. - - - - -	0,963
ditto	VIII. - - - - -	1,234
ditto	IX. - - - - -	1,234
ditto	X. - - - - - - -	1,234
ditto	XI. - - - - - -	1,206

From this it follows, that, in general, tallow candles burn with a brighter flame than those made of wax; notwithstanding that the results of these experiments do not seem to succeed each other in a perfectly regular order. On this account I do not yet venture to infer from these results any decided opinion concerning the strength of light; but shall postpone the fuller investigation of this point to a future time and more leisure *.

* It is hoped the author will then likewise take notice of the thickness of the wicks, as well as of the temperatures, size, and other circumstances of the rooms where his experiments were made, which on one side will make his inquiry more complete, while on the other it will afford a means to form by calculation a reasonable estimate of the probable quantity of such kind of fuel wasted by combustion in a given time, place, and temperature. There is also reason to conclude, that a greater degree of accuracy would have been obtained if the relative illuminations had been determined from the shadows of the same book when equally dark upon an opposite wall, as the intensities of light are then universally as the distances of the candles from the wall.---Translator.

Short

IX.

Short Notice concerning the Properties and external Characters of some new Fossils from Sweden and Norway; together with some Chemical Remarks upon the same. By Mr. D'ANDRADA. *In a Letter to Mr. Beyer, Master of the Mines at Schneeburg.*

SIR,

WHEN you had the goodness to inspect some specimens of the new fossils, which I have found in my last travels in Sweden and Norway, and brought along with me, you desired to become acquainted at least with their number and names. I comply with this suggestion; but wish, at the same time, I were also able to communicate to you a description in my own manner, as well as the results of the analyses which I have already made of some of them, together with that of others, which at present are the object of my occupation, and of those of which Professor Abilgaard has undertaken the analysis at Copenhagen. However, having destined those descriptions and accounts partly for the Academy of Sciences at Stockholm, partly for Copenhagen, partly for the Berlin Society of Friends inquiring into Nature, and partly for the Philomathic Society at Paris, I must for the present deny myself the pleasure of describing them fully.

You will receive, together with this letter, only a few of these species of fossils, together with a short notice concerning their properties and characters.

I. ACANTHICONE.

The colour is leek and olive green, and sometimes, though seldom, that of the greenfinch. Specific gravity from 3.4075 to 3.3562; but that of the more compact sort 3.3000. It is not scratched by quartz, and it gives fire with steel. The texture of the mixed specimens, or in lumps or crystallized, is foliated, but in the others fine splintery, and more compact. The laminæ are thin, very coherent, and of a three-fold crossing.* The entire splinters are quadrangular parallelopipedons with oblique terminations. On the edges it is transparent. The internal lustre is of the vitreous kind. The crystals are, (1.) quadrilateral, hexahedral and decahedral columns, terminated in dihedral, tetrahedral, and hexahedral pyramids; and sometimes also without pyramidal terminations. (2.) Tables or plates, quadrilateral, oblong, rhomboidal, sharpened off on the long narrow lateral facets.

* *Durchgang* in the German. Many fossils are capable of being split in various directions; so that the laminæ cross or tranverse each other under various angles, and hence each particle of such a fossil must belong to one or more laminæ. If these cut each other in one direction only, as in mica, talc, &c, it is called *single crossing*; if in two directions, as in the feld-spar, hornblende, hyacinth, it it called *twofold crossing*; if in three, as in spathose iron ore, ponderous spar, galena, &c. it is then called *threefold crossing*, and so on. See *Emmerling* Lehrbuch der Mineralogie, 1793, Vol. III. 479.---Transl.

If two of thefe plates be rubbed upon each other, they are a little phofphorefcent, and emit a fmell refembling that of quartz by friction. Acanthicone is in fome degree pyro-electrical. Before the blow-pipe upon charcoal it readily fufes, with effervefcence, into a blackifh fcoria replete with air-bubbles. In the cold it is infoluble in the fulphuric and nitric acids. This foffil occurs in the Swedifh iron-mines, near Perfberg, Lengbanfhytta, and Norberg; but in Norway very beautiful fpecimens of it are met with in the Kulftad-mines at Helgoland, and near Arendal in the iron-mines Tornbiornfbo, Ulrica, Not-bro, &c. Till now it was miftaken, or confounded with fchorl, or with green garnets. Some cryftals are fo large as to weigh five pounds.

II. SPODUMENE.

The colour is commonly a greenifh-white of various fhades. The luftre that of the mother-of-pearl. In the fubftance itfelf it is little tranfparent, but much fo on the edges. Its fpecific gravity is 3.218. It fcratches glafs, but is itfelf fcratched by quartz, and affords a white powder. It feels colder than quartz, yet dry and fmooth. Spodumene is not at all electric, nor phofphorefcent; nor does it afford a quartzy fmell by friction. Its texture is lamellar, with a double croffing. Its perfect fragments are rhomboidal acute-angled prifms (120° and 55°). The fragments of the crofs-fracture are for the moft part longifh plates, that have their edges but little fharpened. When fubjected to the blow-pipe upon charcoal, it becomes, at the firft gentle impreffion of heat, opaque, dull, and yellowifh; then developes itfelf in the direction of its laminæ, at the fame time that it fwells a little, falling afterwards into an infipid powder, which by a ftronger heat affords a very tranf-parent greenifh-white glafs. Nitric acid does not diffolve it, nor produce any effervefcence. It is found, together with feveral other foffils, in the remarkable formation of iron at Uton, in Skarrgarde, three miles from Dalero.

III. SAHLITE.

Its principal colour is a pale afparagus green of various fhades. Its external luftre vitreous, inclining to wax, but internally it has little fplendor. It is tranfparent, and fometimes femi-pellucid, if the cryftals are pure, and have fuffered no decay. Specific gravity 3.2368. Sahlite barely marks glafs, and does not ftrike fire with the fteel, and is eafily broken. Its texture prefents ftraight and fmooth lamellæ, thrice croffing each other in a fomewhat acute-angled direction. The fragments of the crofs-fracture are roundifh. Their form of aggregation is of the coarfe-grained kind, and fometimes, if the ftone be in lumps or maffes, they are hamated, or indented into each other. This ftone occurs in cryftals of rectangular, quadrilateral columns, the lateral edges of which are flightly truncated, and convex. The broad terminating edges of thefe columns are flightly trun-cated. The ftreak by rafure is white. This ftone is fomewhat foft and idio-electric. When rubbed upon a piece of the fame kind, it emits no odour, nor fhews any phofphor-

efcence.

escence. Before the blow-pipe it is infusible. It is found in Sweden, in the Sala silver-mine in Westermannland. I have likewise discovered it in Buoen, three quarters of a mile distant from Anen, in Norway, where it occurs in masses, straight and thick foliated.

IV. ICHTYOPHTALME.

The chief colour yellowish-white. Lustre like mother-of-pearl, approaching to greasy. This fossil is transparent. It specific gravity is 2.491. It scratches glass and easily admits of being filed; affording a white powder of a rough feel. Its own substance feels smooth, but not very dry, and is as cold as quartz. It is very difficult to pulverize it. Texture lamellar, of a more than triple crossing. When crystallized the laminæ are large; but they are small when the fossil is massive, or exhibits a splintery fracture. The form of the crystals cannot be determined, as they are very much concreted, and strongly cohere with each other. The fragments of the transverse fracture are irregularly angular, in some instances orbicular with sharp edges. When first acted on by heat upon the charcoal before the blow-pipe, it undergoes no change, not even with respect to its colour; but in the more violent second heat the pointed edges run into a white enamel. With sulphate of lime (gypsum), the ichtyophtalme is infusible; on the contrary, it runs, when mixed with fluat of lime (fluor-spar), into a semi-pellucid milk-white glass, the fusion being accompanied by a little effervescence. This fossil occurs at Uton, in Sweden, and consists of silex and a little alumine, or argillaceous earth.

V. COCCOLITE.

As to colour, coccolite is mountain, grass, and olive-green. Its lustre is vitreous and resplendent. It is opaque, and its specific gravity is 3.316. It scratches glass, but excites only a few sparks with the steel. Its streak is grey, or greyish-white; its texture broad foliated, in which a single crossing of the laminæ is observable. It is an aggregate of large coarse and fine grained, granular polyhedrons. These grains appear in some instances to be short quadrilateral columns, sharpened at both ends, and having pyramidal terminations joined to, or superimposed upon, the lateral surfaces. This stone is infusible by itself. With carbonate of pot-ash it fuses with effervescence, and swells to a frothy scoriaceous glass, of a dirty olive-green colour; and with borax it produces a pale-yellowish semi-pellucid glass. It is met with in the iron-mines Hellesta and Assebro, in Sudermannland, as well as in Nerike, in Sweden, and likewise in a beautiful form in the Arendal iron districts of Norway.

VI. APHRIZITE.

Colour black, somewhat greyish. External lustre vitreous, inclining to the gloss of fat; internally a little resplendent. Aphrizite is opaque. Its specific gravity is 3.1481. It gives fire with steel, and cannot be filed; is very brittle, and readily broken. Its texture is compact. The cross fracture is smooth, a little even, inclining to the flat conchoidal; and

and the fragments of it amorphous, yet angular and of sharp edges. The crystals are hexahedral, short and thick columns, which sometimes, from the different truncatures of the edges, have the appearance of dodecagonal columns. Both species are terminated in tetrahedral pyramids. This stone is feebly idio-electric, but not at all pyro-electric. It intumesces before the blow-pipe on the very first action of heat, frothing at the same time, and yielding a greyish or yellowish white glass. With borax it effervesces, foaming strongly, and produces a greenish white pellucid glass. It occurs at Langoe, a small island not far from Krageroe, in Norway.

VII. ALLOCHROITE.

Allochroite possesses a yellow-grey, and, in some instances, a dark straw-yellow colour. It has little lustre of the vitreous kind, which in the recent fracture passes into that of wax. It is opaque. Its specific gravity is 3.5754. It is just scratched by quartz, gives fire with steel, and is not easily broken by the blow of the hammer. Its texture is compact, and it is met with in large, thick slaty plates, with a decayed yellowish-white surface. Fracture uneven, of the small and perfect conchoidal kind. Fragments angular and indeterminate, not much sharpened at the edges. It is infusible alone as well as with borat of soda (borax). When treated with microcosmic salt (phosphate of soda and ammoniac), it exhibits an enamel-like, more or less perfectly fused surface, which, on gradual cooling, shews at first a reddish-yellow, then a green of differently deep tints, and at last a dirty yellowish-white colour. This change of colours seems to indicate some metallic ingredients. The native place of this stone is the mine Wirum, in the vicinity of Drammen, in Norway.

VIII. INDICOLITE.

The colour of this stone is a dark indigo-blue, a little lighter in the fracture, so as to incline to the azure or sky-blue. Its external lustre is vitreous in a high degree, approaching to the metallic splendor. It is untransparent, and not very heavy. Its specific gravity cannot be accurately ascertained, on account of the small crystals bedded in it. Quartz is a little scratched by it. It is easily broken. The streak is blueish-grey. It feels cold and dry like feld-spar. Its texture appears to be compact; but the longitudinal fracture is finely striated, and the cross fracture somewhat uneven, passing into the small conchoidal. Its crystals are rhomboidal columns, much striated lengthways. The fundamental form of crystallization seems to be quadrilateral; but for the most part these crystals are polyhedral, needle-shaped and stellular. It does not fuse before the blow-pipe. It is found near Uton, in Sweden.

Note. This fossil resembles in its colour the lazulite of Professor Klaproth, which I know only by description: but as to its other physical and chemical characters it differs from it.

(To be concluded in our next.)

X.—*Experiments*

X.

Experiments on the Conducting Power of Fluids with Regard to Heat.—W. N.

AMONG the papers presented to the philosophical world since the commencement of the present Journal, none occupy a higher rank than the experiments and deductions of Count Rumford on heat; whether we attend to their extensive and important consequences, or the powers of philosophical investigation they exhibit. One of the most singular facts he has noticed is the slowness with which heat is conducted through fluids; a fact long ago pointed out and strikingly impressed by him, and latterly rendered more remarkable by his inference, that bodies in the state of fluidity have absolutely no power of conducting heat from particle to particle, but convey it from one solid to another merely by their motions or currents. Whether this inference taken in the extreme of strictness could be justified by his own, or indeed by any experiment, was doubted by myself as well as others; but without treating the subject in that precise manner, it becomes a question of some interest and curiosity, to decide whether the direct conducting power of fluids be too minute to be ascertained by careful experiments more or less varied from those of the Count. Doctor Thomson, of Edinburgh, is, I believe, the first who undertook and conducted a process of this kind, an account of which is given in his able paper in our last volume, p. 529. I need not here advert to the particulars of that memoir, farther than to remark, that in the discussion of this subject in conversation by some philosophical friends who meet weekly at my house, our consideration was more particularly directed to the argument which is drawn at p. 534, in favour of the condensing power of fluids from the Count's own experiments. It is there observed. that when ice was slowly melted by boiling hot water standing over it, with the interposition of a thin stratum of ice-cold water, there were undoubtedly currents produced through this last stratum, because water becomes denser either by receiving or losing heat till it arrives at 40°; but it is contended, that the heat which passed to the cold water in this process must have been conducted from particle to particle, and not conveyed by currents of the superior water; and farther, that the regular diminution of the numbers expressing the temperatures of the strata of water from the surface downwards, is inconsistent with the supposition of such currents, which it should seem ought to produce a mixture of the fluid.

It appeared to us, that the principal question in this case was, whether or not the whole conveyance of the heat were effected by the mediation of the vessel or solid; which it ought to be if the solid be the only proper and perfect conductor present? That is to say, if heat be applied to the surface of a fluid, and the effect of that heat be to produce rarefaction, it must inevitably follow, in this simple statement of the case, that no heat would pass below the range of particles at the very surface where the heat was applied, unless the fluid itself were properly and strictly a conductor.

But

But as the cafe is not fo fimple, but muft neceffarily include the veffel, or folid coating of the fluid mafs, the confequences will be different. If the fluid be capable of receiving and giving heat only from and to folids, we muft contemplate the heated parts as rarefied, (that is to fay increafed in bulk) and confequently diffufing themfelves laterally over the reft of the furface; as coming into contact with and heating the veffel; as urged to defcend at the circumference by the fucceffive arrival of other more heated particles, and confequently converging again beneath, and afcending near the center: we muft confider the folid fide as conducting the heat ftill farther down; and there caufing an afcending current to join its water to the converging ftreams above. So that the water may be confidered as receiving heat from a folid in contact with its furface near the center; giving it again to a folid at its circumference; and this laft folid in its turn heating another ftratum of water beneath. As the veffel fhould become heated to a greater diftance downwards, the effect would be more perceptible to a greater depth in the fluid; and from the want of communication between the particles among each other, the afcents and convergences might have different velocities, according to the temperatures at the feveral depths, and preferve different temperatures in the feveral ftrata regularly increafing according to the advancement of the operation.

We thought it probable, as the paffage of heat downwards is very flow, that thefe currents, if they exift, might alfo be fo flow that different temperatures might be found at one and the fame time in the fame ftratum; and we fuppofed the experiment would be more conclufive, if the heating folid were made to cover the whole furface of the fluid, and to reft upon the veffel itfelf. For in this arrangement, as the upper edge of the veffel would be as hot as the furface of the fluid, we concluded that there could be no divergence, but that a fimple afcending current would be produced near the fides, with a defcending one near the axis of the veffel. We chofe a veffel of wood, as the worft conductor we could make, * for obvious reafons.

Plate X reprefents the apparatus, A B is a ftage for philofophical experiments contrived by Profeffor S'Gravefande, C is a cylindrical veffel of mahogany of the following dimenfions; outfide diameter 4.75 inches, outfide height 2.75 inches, infide diameter 3.5 inches, infide depth 2.0 inches. In the upper edge, which was confequently 0.625 inches thick, there was a cylindrical excavation to the depth of 0.1 inches, and of the diameter of 4.5 inches. In the bottom of this veffel were fixed with fealing wax, two thermometers L and M, the one in the axis, and the other about 0.2 inches from the infide furface, and the bulb of each ftood 0.75 inches higher than the inner furface, or bottom of the veffel; confequently the clear depth of the veffel from the upper edge to the thermometer, was 1.25

* A cylinder of glafs 1 inch in diameter, and 0.25 inches thick, was placed on the horizontal cover of a veffel containing water kept boiling by a lamp, and a like cylinder of light dry mahogany was placed befide it at the fame inftant. A very minute cone of tallow ftood on the center of each piece. That upon the glafs fell down by fufion in one minute; that upon the wood in two. This method is much more accurate than that of Franklin with long rods defcribed in Dr. Ingenhousz's Effays.

inches.

inches. A plate of ſteel I, four inches in diameter, and 0.3 inches thick, was uſed as the cover for the veſſel; the remaining half inch of the excavation being left to receive the fluid, which would riſe by expanſion when heated. D is a tin veſſel, of which the dimenſions may be ſufficiently known from the figure. It has a looſe cover or valve at E, and is cloſed every where elſe. At H a branch proceeds, which ends in a cylinder F; and at G is a ſcreen, to prevent the heat of the lamp K from affecting C; in addition to which, during the experiment, there was placed a paſteboard ſcreen upon the ſtage near the letter K. It will now be eaſily ſeen that when the veſſel C was correctly filled (except the excavation at top) with any fluid, and the ſteel cover laid in contact with the ſurface, and the veſſel D partly filled with boiling water, and then placed in its ſituation here ſhewn, and, laſtly, the ſpirit lamp K placed beneath the outer end of the branch H,—the temperature of 212° would be conſtantly applied to the ſteel, and the ſurface of the fluid accordingly heated. Whence if any heat proceeded downwards, it would be ſhewn by the thermometers; and if ſlow currents of different temperatures prevailed ſo as to produce the variation before ſtated, theſe thermometers would differ. It was thought that ſuch a difference would prove the exiſtence of currents, and the probability that the wooden veſſel was the chief agent of the conduct; but it was not ſo clearly admitted that their agreement would prove the contrary poſition.

Brevity prevents my relating our arguments and views, and the nature of unpremeditated converſation, as well as other motives, forbid me the honour of mentioning the names of the philoſophers to whom I refer in this diſcourſe. I ſhall likewiſe, for the ſake of conciſeneſs, omit ſeveral experiments which were made with other apparatus of leſs ſimplicity, or leading to the ſame facts. One, however, with water I may curſorily notice. It was made in a glaſs veſſel, with a cork bottom, nearly of the dimenſions of the wooden veſſel here deſcribed. During the time the boiling veſſel E was kept in contact with the ſurface of the water, minute bubbles aſcended ſlowly from the ſeveral parts of the water, which appeared to indicate that there was no perceptible current. A quantity of elaſtic fluid in bubbles occupied the ſurface of the liquid, and prevented its contact with the veſſel. The thermometers were raiſed conſiderably, but ſlowly, as was the caſe in many other experiments with water; but as theſe require to be repeated, I omit the reſults.

Several trials were made with mercury and the apparatus in the Plate. The following was made with much care:

Pure diſtilled mercury was put into the veſſel, and left undiſturbed, with the ſteel cover upon it, for upwards of an hour. The veſſel D was then partly filled with boiling water, and carefully placed on the ſteel cover, and the lamp K lighted. This was at thirty-two minutes paſt eight. The concluſion of the experiment was effected by lifting off the veſſel D by the hand covered with a glove.

Time.

Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.
$8^h\ 32^m$	$75\frac{1}{2}°$	$75\frac{1}{2}°$	37^m	88°	88°	$41\frac{1}{2}$	105	105
33	77	77	$37\frac{1}{2}$	89	89	$42\frac{1}{2}$	106	107
$33\frac{1}{2}$	78	78	38	91	91	43	108	$109\frac{1}{2}$
34	79	$79\frac{1}{4}$	$38\frac{3}{4}$	94	94	$43\frac{3}{4}$	111	112
$34\frac{1}{2}$	80	$80\frac{1}{4}$	39	95	95	$44\frac{1}{2}$	113	114
35	81	82	$39\frac{3}{4}$	97	97	45	115	116
$35\frac{1}{2}$	83	$83\frac{1}{2}$	40	98	98	$45\frac{3}{4}$	117	118
$35\frac{3}{4}$	84	$84\frac{3}{4}$	$40\frac{1}{2}$	100	100	46	118	$118\frac{1}{4}$
$36\frac{1}{2}$	86	$86\frac{1}{2}$	41	103	103	$46\frac{1}{2}$	120	$120\frac{1}{4}$

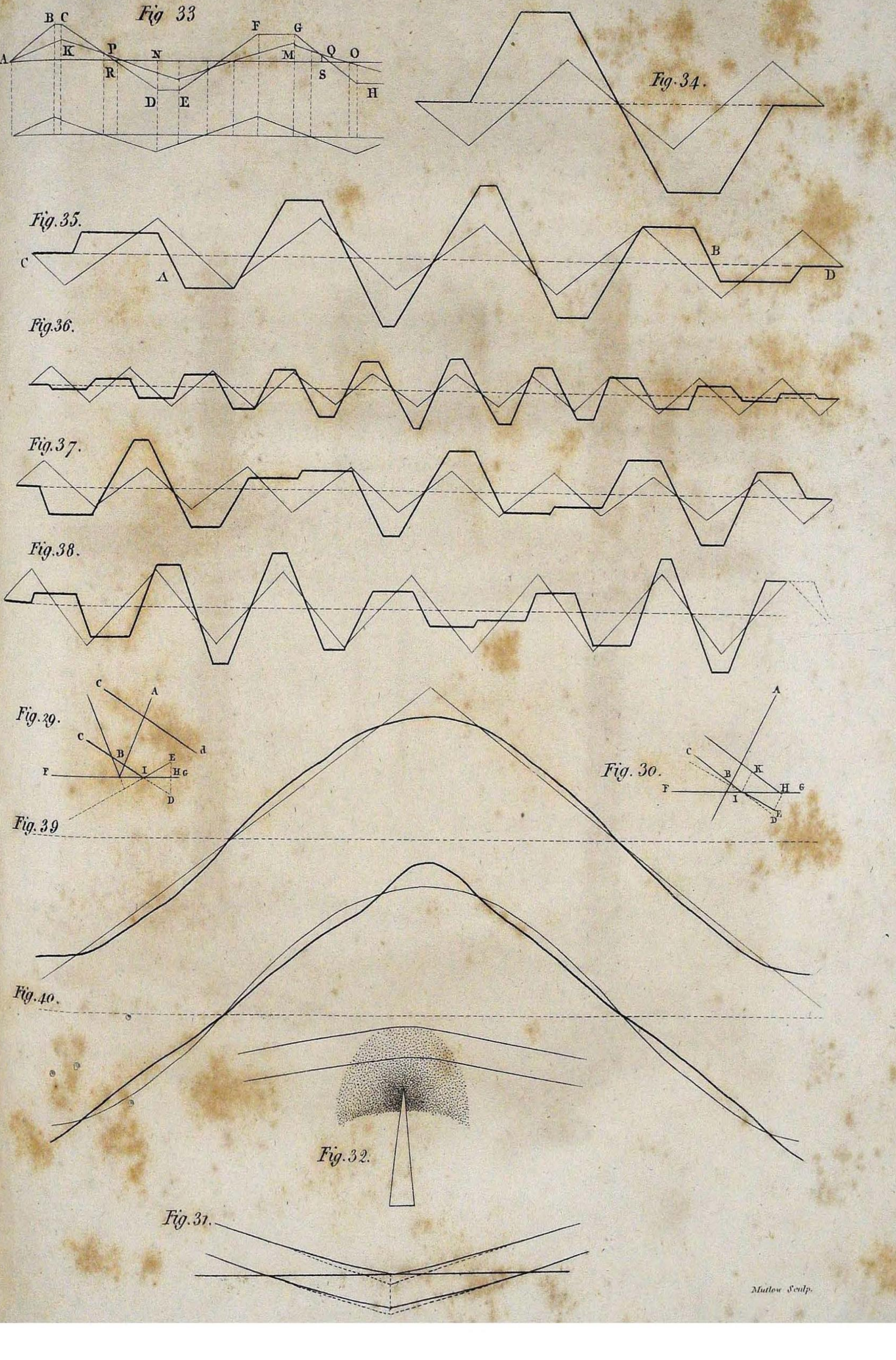
Another wooden veſſel, exactly of the ſame dimenſions, and fitting up, was then filled with olive oil, and left with its ſteel cover on for a ſhort time, and then the boiler and the lamp were placed. It is to be obſerved, that the lamp was very ſufficient to keep the water rapidly boiling over the water in the glaſs veſſel; that it boiled but moderately over the mercury; and with much noiſe and tremulous action over the oil. This, and the difference of time employed in heating the different fluids, muſt ſhew either a difference of facility in circulation, or in receiving and giving, or in abſolute conducting power in equal bulks. The mercury, it is ſeen, required ſeven minutes to raiſe the middle thermometer through twenty-one degrees, from 77° to 98°; but the oil required thirty-four minutes, or was nearly five times as long in conducting or conveying the ſame heat.

Table of the Experiment with Olive Oil.

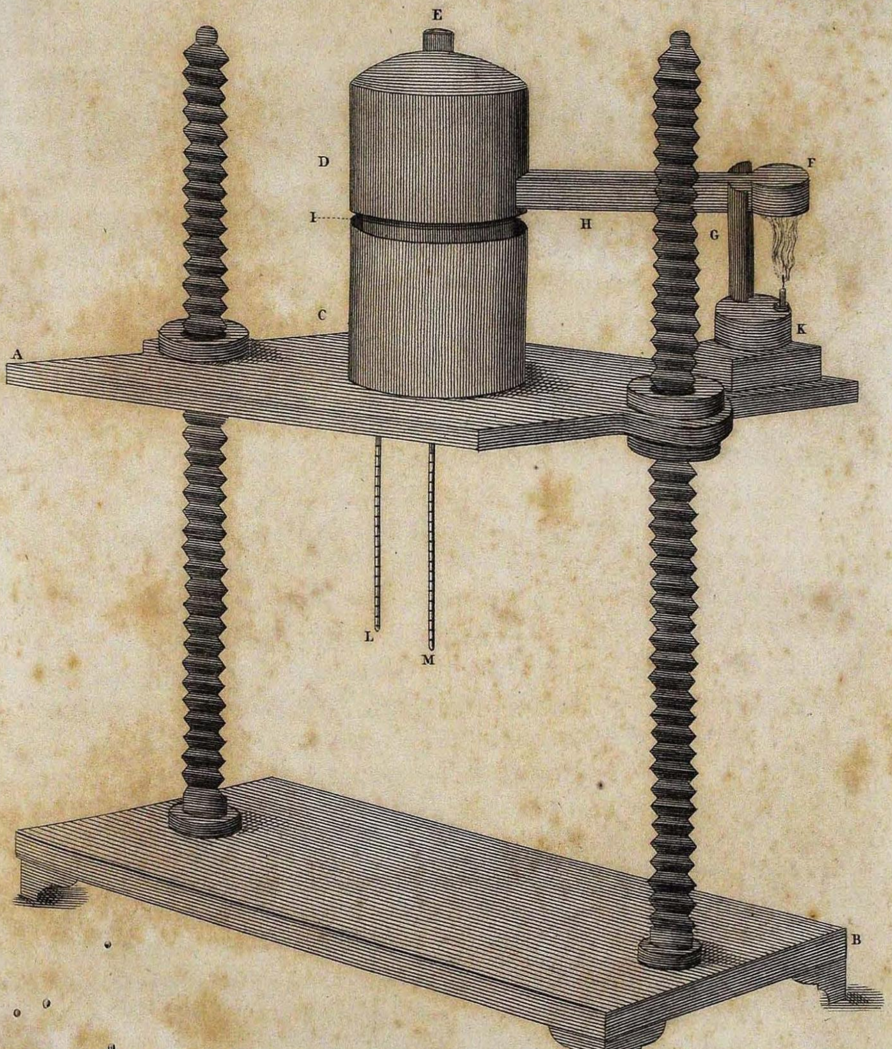
Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.
$9^h\ 7^m$	72°	72°	$33\frac{1}{2}^m$	78°	79° +	$41\frac{3}{4}^m$	$82\frac{3}{4}°$	84°
16	73 —	73 +	$34\frac{3}{4}$	79	80	44	84	$85\frac{1}{2}$
$22\frac{1}{2}$	74	$74\frac{1}{8}$	37	80	81	$56\frac{1}{2}$	90	92 —
27	$75\frac{1}{2}$	76	38	$80\frac{3}{4}$	82	61	92	94
31	77	78	$40\frac{1}{2}$	82	$83\frac{1}{3}$	75	98	100

While this experiment was making, another boiler was kept in action upon a glaſs cylindrical veſſel filled with olive oil. An obſerver carefully noticed the ſmall opaque particles ſuſpended in the fluid, but he could pereceive no current during the continuance of thirty minutes.

On Light & Sound. (see pages 72, 81, 121)



Transmission of heat thro' fluids downwards.



Mutlow Sculp.